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ORIGINAL ARTICLES.

SOME RECENT ANNOUNCEMENTS OF PASTEUR CONCERNING HYDROPHOBIA.

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FOR the same length of time which Pasteur has spent in laboratory experiments with rabies, I have been engaged in a special study of the same subject; at the bedside, and in what has been put on record about it by practical physicians and experimenters from the time of Aristotle to the present day. At the beginning, I had no prejudice and no pet theory in regard to hydrophobia; but as my studies advanced, certain ideas, quite different from those with which I began, impressed themselves more and more deeply upon my mind, until they developed into strong convictions. It ought not to seem strange that this should be the case; nor, I think, ought these convictions—which are the result of a long and, I trust, conscientious search for the truth—to be regarded by those who do not share them, and who can hardly have studied this subject as extensively as I have, as if they were mere prejudices. To err is human; and merely to have read and observed and thought much about anything, gives no guarantee of infallibility. But this method is generally more reliable than the most brilliant intuitions; and no matter how eminent may be any individual experimenter or theorist, his own work must be investigated and compared with that of others, before a just estimate of it can be formed by those to whom his conclusions are offered for acceptance. Pasteur is only one of hundreds of men who have studied the subject of hydrophobia experimentally, and his announcements have no more claim to acceptance without investigation than have those of any of the many who have preceded or who have followed him. When prizes are to be awarded, it is often made one of the conditions of competing that each work shall be known only by some indifferent mark or legend, so that no dazzling reputation shall blind the judges to its demerits or add a borrowed brightness to what is in itself excellent. This admirable principle should not be forgotten in judging Pasteur's work. If it be trustworthy, it needs none of his great fame to establish it; if it be untrustworthy, this fame may come to serve an inglorious purpose. With these ideas fixed in my mind, I have carefully studied all of Pasteur's communications about hydrophobia as they appeared, and have subjected his theories and the evidence which he has adduced in support of them, to the critical tests which I think should be applied to every new announcement in the realm of science. If Pasteur or his friends think that to dissent from his opinions is to cast discredit upon him, I do not see how they can be driven from this position. But the fair critic would be glad to distinguish between a man's purpose and his work, and to honor a good intention, even when his sober judgment will not permit him to endorse the steps taken to carry out such an intention.

It cannot be denied that the determination to discover a means of preventing the outbreak of so dreaded a malady as hydrophobia is a noble determination; but it does not follow from a recognition of this fact that we must blindly approve all the methods or conclusions of one who is animated by so admirable a motive. We may, therefore, grant all that is claimed for the humanitarian purpose of Pasteur, if we keep constantly and clearly before our minds the fact that it has no bearing whatsoever upon the correctness of his opinions. These must stand or fall solely upon their own merits.

I have already, in a paper read before the Philadelphia County Medical Society, January 13, 1886 ("Comments on Pasteur's Method of Treating Hydrophobia," *New York Medical Record*, February 13, 1886), passed in review the claims of Pasteur in regard to his preventive inoculations against hydrophobia, and the evidence in support of them. I cannot now repeat what I then said, but would refer those who care to study in detail the grounds for my estimate of Pasteur's claims at that time, to that paper. The estimate, however, I desire here to repeat and to emphasize. It was: that Pasteur's so-called method of treating hydrophobia before the outbreak appears to be founded upon untrustworthy experiments and unsound reasoning, and ought to be rejected and condemned, in the interests of humanity as well as of science.

Since the time when I expressed this judgment, events have occurred which have confirmed it in my mind.

Pasteur has made three new communications to scientific bodies. The first of these is entitled, "Resultats de l'application de la méthode pour prévenir la rage après morsure" (*Bull. de l'Acad. de Méd.*, March 2, 1886, pp. 294–303). (This was also communicated to the Académie des Sciences, and is published in the *Comptes Rendus*, of that body.) In this paper, Pasteur claims to have treated successfully—with one exception—up to February 25, 1886, 350 persons bitten by rabid dogs; and ends with the exclamation, "The prophylaxis of hydrophobia is established" (*La prophylaxie de la rage est fondée*). In April, 1886, he made his second communication, entitled "Note complementaire sur les resultats de l'application de la méthode de prophylaxie de la rage après morsure" (*Comptes Rendus de l'Acad. des Sciences*, April 12, 1886, pp. 835–838). In this paper he says that he has treated 726 cases. Pasteur's third communication was made to the French Academy of Medicine, on May 4, 1886, and

is entitled "Sur les resultats de l'application de la méthode de prophylaxie de la rage" (*Bull. de l'Acad. de Méd.*, May 4, 1886, pp. 664, 665). In this communication he says he has treated 950 cases, with only seven deaths.

At first sight, these statements seem to justify fully the enthusiastic applause with which Pasteur's announcements have been received by many of his countrymen, and the hopes which they have excited throughout the whole civilized world. But, proud as the French may be, and hopeful as all the world may be, reason will have its turn, and the critic must discard both pride and the most sanguine expectations when he studies the foundation for either.

Let us then consider Pasteur's announcements dispassionately and critically. Theoretically, I think them diametrically opposed to reason and analogy. I have already (in the paper before alluded to) attempted to show that Pasteur's method for the prevention of the outbreak of hydrophobia after inoculation from a rabid animal is utterly inconsistent with what he has claimed in regard to the prevention of other inoculable diseases, inconsistent with the work of other experimenters, and inconsistent with the natural history of hydrophobia. I cannot refrain from calling your attention once more to the fact that Pasteur has never given the slightest evidence of being familiar with any work done outside of his own laboratory, or with the history of his subject. He may be granted to know what are the symptoms of the disorder which he can provoke or prevent by the inoculation of his chosen material, and his ability to provoke or prevent this disorder need not now be discussed. But, I assure you, I have not found any reason for believing that this disorder is rabies, or that Pasteur knows what the rabies which has been discussed from the time of Aristotle, 350 years before the Christian Era, to the present day really is. I do not find in Pasteur's statements anything clearer or more rational than is embodied in the crudest popular ideas in regard to hydrophobia, and his artificial disease does not appear to me to have any definite relation to it.

But, to come to his so-called facts. Pasteur claims to have treated the enormous number of 950 cases, between July 7, 1885, and April 12, 1886—a period of only nine months. In regard to them, he claims to have proof that all his patients had been bitten by rabid animals. If this were true, we might, perhaps, feel justified in shutting our eyes to the theoretical objections to Pasteur's method, and rejoice in the triumph achieved over the dangers which flow from such bites. But, unfortunately, we cannot believe that it is true. In the first place, never in the history of the world has anything like such a number of cases of exposure to hydrophobia been observed. I cannot repeat the mass of statistics which I have studied. The best statistics show that before the invention of Pasteur's method there were annually from 12 to 15 victims of hydrophobia in France. At the rate of the "cures" which Pasteur claims, there would be annually about 4000! The statistics of all countries show about 1 to 1½ per million inhabitants. In Austria, from 1879 to 1885, there were from 13 at most to 2 at least in a year; while in Prussia the deaths fell progressively from 10 to 6, 4, 1, and 0! (See Lutaud: Questions à M. Pasteur. *Journ. de méd. de Paris*, May 2, 1886, pp. 683–692.) If anyone wishes to assert the contrary, let him bear the burden of the proof. Pasteur himself has not undertaken it, but has attempted to explain the unparalleled number of cases he has treated by saying that the cases were suppressed by denials of danger and wilful deception of persons bitten by mad dogs in former times. To this it may be replied that, if such a course has kept the general mortality down to about one or one and a half per million per annum —as something has done—it may be as humane a method as Pasteur's preventive inoculations. But we all know that such a course would never have had such results if the dangers were of the nature and gravity which Pasteur assumes for them. It seems, indeed, as if the attention attracted to this disorder by the announcements of Pasteur had led hundreds of persons bitten by dogs entirely innocent of rabies to hurry to his laboratory to be cured of a disorder which they would never have thought of but for his own influence upon their minds. This conviction is strengthened when we study the cases with which we are familiar. Shall we, in America, forget the Newark children, who figure in Pasteur's list of cures? Six were bitten by the same dog. Four went to Pasteur, and came back "cured;" two staid at home and did nothing, and are just as well as the four. And the dog that bit them was not mad, either, as I felt sure and said at the time—for I have made a study of rabies in dogs, also. No human being or lower animal bitten by this dog—which was incontinently killed—has given evidence of rabies. Dr. Anna Kingsford, of England, has taken the trouble to investigate seven other cases of Pasteur's best known and most widely cited "cures," and has found them all void of solid foundation (see *N. Y. Evening Post*, March 30, 1886), and very pertinently concludes that when the cases which are made so much of turn out to be so worthless as proofs of the efficacy of Pasteur's method, it is hardly worth while to inquire into the accuracy of less important "cures," concerning which the necessary data are not accessible. Certainly Americans cannot supply what is needed to support Pasteur's claims.

Again, I must direct your attention to the cases of death of bitten persons while under Pasteur's treatment. His first fatal case was a little girl brought to him thirty-six days after being bitten, who died the same day, I believe, on which Pasteur declared, "I am confident my treatment will be successful, if commenced any time before active hydrophobia sets in, even if a year or more elapses between the bite and the commencement of the treatment." He was mistaken then—perhaps he is mistaken in what still remains of his confidence.

But, then, he has since had six deaths under his treatment. These he explains by the fact that the patients had been bitten by wolves, and the *assumption* that the bite of a so-called rabid wolf is more dangerous than that of a so-called rabid dog. Here is a plain begging of the question. He assumes just what he ought to prove. And his assumption is absolutely unfounded in fact. There is no reliable

evidence in support of it. He himself has not found any. Pasteur attempts to support his assumption by citing five apocryphal instances, in the years 1706, 1806, 1812, 1849, and 1866, in which the mortality after the bite of supposed wolves was nearly one hundred per cent. But these instances are too untrustworthy to merit serious attention; and, as Lutaud has pointed out in an admirable article ("Questions à M. Pasteur," *Journal de méd. de Paris*, May 2, 1886, p. 689), Pasteur "ought to give us recent cases, or at least cases of a less legendary date, so that they might be carefully investigated."

In fact, there has never been any person before Pasteur who thought rabies in wolves differed from the same disease in other carnivora, and it furnishes no support to this notion, that it should have originated with Pasteur, and least of all, that it should have originated under the pressure of circumstances so unfavorable to his method for combating hydrophobia.

If we sum up what has taken place during the first nine months of the application of Pasteur's method of preventing hydrophobia, we find that there has been an enormous increase in the number of persons who believe they have been bitten by mad animals, and a decided increase in the number of deaths from hydrophobia. In France, England, and America where Pasteur's announcements have been most warmly received, this increase of fear of death has been most marked. In Germany, where there is a national prejudice against all French ideas, the new teachings in regard to hydrophobia have not taken root, and there has been a blessed freedom from the panic and folly which have been exhibited in the three countries I have named, and hydrophobia is almost unknown.

I cannot, for my own part, escape the conviction that the differences between what has taken place in France, and what has taken place in Germany, must depend upon the different attitudes assumed toward Pasteur's theories, and that the result of Pasteur's work thus far must be considered to be altogether unfortunate. So far from diminishing the ravages of rabies and hydrophobia, it seems to have increased them.

I, therefore, now repeat, in all soberness, and with a full appreciation of the responsibility of such an utterance, what I publicly stated as my conviction last January, namely, that Pasteur's method of treating hydrophobia appears to be founded upon untrustworthy experiments and unsound reasoning. And I now add to this my further conviction, that its practical results have been an increase in fear and death, and I think more strongly now than I did six months ago, that this method ought to be rejected and condemned, in the interest of humanity as well as of science.

CLINICAL OBSERVATIONS ON FIBROMYOMATOUS TUMORS OF THE UTERUS.[1]

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AMONG a limited number of the fibromyomatous tumors of the uterus which have altogether come under my observation, I had the opportunity in three instances to see a rather uncommon termination of the disease, namely, in two cases by expulsion of tumor-masses after spontaneous sloughing; in the third case by shrinkage of the tumor after central suppuration and softening. In all of these cases more or less surgical help became necessary to aid the natural process, but all of them, in spite of protracted serious illness, at last ended with recovery, and illustrate, in a very eloquent manner, the ability of nature to find, in spite of serious obstacles, its way toward ultimate recovery. The following short histories may sufficiently point out the essential features of these cases:

CASE I.—On the 12th of October, 1883, I saw in consultation with Dr. Schaide, of this city, Mrs. B., then forty-five years of age, who was suffering from a large abdominal tumor, which had existed for about three years, and by a number of physicians, both here and abroad, had been diagnosticated as fibromyoma of the uterus. To Dr. S. I owe most of the following notes about her history. Mrs. B. had been advised everywhere not to have an operation done on account of the great risk attached to it. Within the last year she had been treated repeatedly for long periods of time with ergot, administered hypodermically as well as internally, but without any noteworthy success, in reference to hemorrhage as well as size of the tumor. The latter, when I saw the patient, filled, as a resistant, somewhat irregularly shaped mass, almost the entire abdomen. Having at that time already operated successfully upon several patients with exceptionally large solid uterine tumors, I proposed, in view of the intense suffering of the patient, the radical operation. The patient declined. In the beginning of November her condition became feverish, there appeared an offensive discharge from the vagina, and the patient lost rapidly in flesh. On the 18th of November Dr. S. was able to remove a piece of the tumor about the size of the fist, in a decomposed necrotic condition, from the vagina, which, for the next two weeks was almost daily followed by others of smaller or larger size. A particularly large piece was extracted from the uterus at the end of November. I then saw the patient again in consultation. To my surprise, the enormous tumor had so much disappeared that the uterus now was not larger than at about the fourth or fifth month of pregnancy. There existed still a very offensive discharge, which, however, ceased very promptly, after I had extracted from the uterine cavity the remainder of an entirely separated mass of tumor. The offensive smell of this sloughed tissue was beyond description. The uterine cavity was disinfected as thoroughly as possible, and disinfecting irrigations kept up for some time after. The patient, though very much run down, made a rather rapid recovery and was able, four weeks later, to see Dr. S. at his office. The latter had the kindness to inform me that Mrs. B. is now in perfect health. About a year ago her menses, which had become quite normal, ceased. There is no recurrence of the tumor, and the parts seem to be in an almost normal condition. So far as I remember, the mass of the tumor at my first visit must have weighed at least fifteen to twenty pounds.

CASE II.—Mrs. H., forty-six years of age, who had never been pregnant, consulted me in October last. Though otherwise in good health, she had suffered for the last two years from profuse and prolonged menstruation, now and then associated with severe pains in her back and vomiting. Within the last month, a few days before and after menstruation, a white discharge had appeared. There was no doubt that her trouble was due to a fibromyomatous tumor of such size that

[1] Read before the New York Surgical Society, May 24, 1886.

the uterus, on examination, reached within about two fingers' breadth of the umbilicus. During the time from the 12th of October to the 6th of November about eighteen hypodermic injections of Squibb's fluid extract of ergot were administered in the hypogastric region. They caused a good deal of pain and inflammatory irritation, which, however, by cold applications was kept down, so that no abscesses occurred. Small indurations, however, remained at the points of injection. During the menstrual period the patient rested at home, did not get any hypodermic injections, but took the ergot internally, fifteen to twenty drops, twice a day. On the 14th of December I was called to see Mrs. H. at her house. She had a bloody, somewhat offensive discharge from the vagina, and suffered from great restlessness and pain of labor-like character. The vaginal portion of the uterus was softened and dilated, and a soft mass could be felt within it. On the following day, under chloroform, a considerable mass, about a pound and a half, of sloughed fibromatous tumor was removed from the uterine cavity, after lateral incisions into the vaginal portion had been made. On account of the narrowness of the sexual passages and the impossibility of pulling down the uterus, I could not pass my finger high up into the uterine cavity. But I was able to ascertain that a good many necrotic irregular pieces of tissue remained undetached as yet. Two drainage tubes were introduced into the uterus, and repeated irrigations made with a warm solution of salicylic and boracic acids, and once or twice a day of corrosive sublimate solution 1 : 5000. But in spite of my endeavors, I did not succeed in preventing further decomposition. The drainage tubes were so often closed by small particles of necrotic tissue that they did not act satisfactorily. Besides that, their presence in the internal orifice seemed to be the source of constant irritation. I therefore removed them and applied irrigation several times a day, by passing a Fritsche's uterine irrigator high up into the uterine cavity, usually injecting, first a stronger solution (1 : 2000) of corrosive sublimate, followed by injection of bor-salicylic acid, according to Thiersch's prescription.

Very soon, under repeated chills and high fever, the palpable symptoms of peri- and para-metritis set in. Especially in the cavum Douglasii, where a fibroma, inserted at the supravaginal portion of the cervix, was felt before a diffuse infiltration and exudation could be made out, pushing the lower portion of the uterus toward the symphysis. Dr. Noeggerath was called in consultation, and was likewise convinced of the extremely precarious condition of the patient. It was decided to renew the attempt to remove the source of infection from the uterine cavity, and on the 23d of December, in the presence and with the kind help of Dr. Noeggerath, sloughed masses, much less, however, than the first time, were again removed, partly by forceps, partly by curette. It was discovered that from the anterior as well as from the posterior aspect, tumor-like prominences protruded toward the uterine cavity, and that the process of sloughing was particularly seated posteriorly and toward the fundus. The uterus, by this time, had already become considerably reduced in size. On the 5th of January I made a deep incision into the exudation through the posterior cul-de-sac, evacuating pus mixed with small pieces of necrotic tissue, as I supposed, from the centre of a fibroma which had undergone suppuration, and from that time, while the discharge from the uterus became gradually less, and almost daily small pieces of necrotic tissue were expelled, the patient's condition became decidedly better. Several times slight hemorrhage accompanied the expulsion of sloughs. At present, for more than a month, the discharge has entirely ceased. The uterus is very little larger than its normal size. There is still a decided induration at the seat of the para- and peri-metritic inflammation, which, however, is gradually diminishing. The general condition is very good, and twice already, if I am not mistaken, the menstrual flow has appeared again, normal in quantity and duration. Mrs. H. is now forty-seven years of age, and certainly very near the end of her menstrual life. I think there is very little probability that a new formation of fibromyomata will occur. I am under the impression that in this case the whole mass of the tumor had not sloughed, but that several lumps, together with the gradual contraction of the uterus, have disappeared either by atrophy or fatty degeneration. It seems, further, that in this case the administration of the ergot had a causal relation to the necrosis and elimination of the main part of the tumor.

CASE III.—Miss B. L., twenty-eight years of age, was in good health until four years ago, when she began to suffer from profuse menstrual hemorrhage and pain in her back, which were ascribed by a physician as due to the presence of a uterine tumor. For about three months she was treated with hypodermic injections of ergot, altogether about thirty in number, but without success. Three years ago, in making a forced attempt to prevent herself from falling down by throwing herself backward, she felt a severe pain in the abomen, and had the sensation as if something had been torn; the pain in her abdomen persisted during the summer. Ergot, internally was used again, but gave her no relief. In September, 1883, I saw the patient in consultation with Dr. Dieffenbach, when she presented a deep-seated phlegmon of the abdominal wall, apparently in the retroperitoneal space below the umbilicus. On the 15th of September, through an incision in the linea alba, a great quantity of pus was discharged; a drainage tube was passed at one point to such a depth, into a rather small appendix of the cavity, as to allow of the conclusion that it must reach to some distance into the abdominal cavity. After some time, when infiltration and pain had sufficiently ceased, it could be made out that a uterine tumor, originating from the fundus of the uterus, was adherent to the abdominal wall, and that a pus cavity passed to some distance into its mass. Suppuration went on for several months, and the tumor gradually decreased in size, having presented originally about the dimensions of a small child's head. Small, irregularly shaped calcareous masses were repeatedly washed out or extracted from the bottom of the wound. On the 31st December, 1883, after having enlarged the existing opening, I removed from the rather narrow cavity by scraping, elevator, forceps, and finger, quite a considerable quantity of calcareous spiculæ, shells, and irregular shaped bodies, in all perhaps as much as a tablespoonful. Four weeks later cicatrization was complete, and since then the patient has enjoyed perfect health. A hard lump in connection with the scar, about the size of a duck's egg, can be still felt, but does not cause the slightest inconvenience, while menstruation has at all times been normal. I cannot say what connection exists at present between the tumor and the uterus, the patient not having undergone an examination of sufficient thoroughness. It seems that in this case, through an injury, perhaps by partial rupture of the insertion of the tumor, its nutrition has been interfered with until finally central softening with suppuration occurred, which gradually led to its diminution and arrest of growth and development. No medicinal treatment was used after the operative interference.

Though quite a number of cases are on record in which, after the manner described in the preceding histories, fibromyomatous tumors have disappeared, they are rather exceptional, and withal this natural way of healing is not free from danger, no small percentage of cases ending fatally. We are, therefore, in no way entitled to trust to such an exceptional and unreliable course of the disease so far

as to give it any important weight in regard to our prognosis of fibromyomatous tumors of the uterus. We know only too well that sometimes these tumors will have an unbounded development, and, after they have attained a certain size, become dangerous, and in an imperative way demand our surgical help, and that so much more as in these cases medicinal treatment is usually nothing but loss of time.

With reference to the question, how far the extirpation of the ovaries may effectually check the development of fibromyomatous tumors, I am unable to give a satisfactory answer from my own experience. I have attempted the operation once, but only to find out that in that case the removal of the ovaries would have been a very tedious operation, and probably not much less dangerous than the removal of the whole mass. In those five cases in which, so far, I have seen the necessity of performing supravaginal amputation of the uterus for the disease in question, the tumors were of such uncommon size, and were mostly so complicated by adhesions, that it would have been no easy thing at all to get at the ovaries, and this, together with the question whether, in such cases, castration promises the desired result, has led me in all those five cases to give preference to the radical method of operating. All have ended in recovery; two of them have been reported to this Society in previous years, one being complicated with pregnancy; the third one was operated upon about a year and a half ago; of the last two cases which have been operated upon in the course of this year, I present before you the tumors removed. With reference to the third of the before-mentioned cases, I would say that it was that of a married lady of about thirty-three, who for several years at every menstrual period had bled so abundantly as to become quite anæmic, her whole way of living finally being devoted to the purpose of building herself up to withstand the drain of the next menstruation. She suffered repeatedly from very alarming attacks of weakness of the heart, which seemed to be dilated in its right half, and there was no doubt that, every other remedy having proved ineffectual, she would finally have died from the consequences of a fibromyomatous degeneration of the uterus, which, as the specimen afterward proved, consisted of a large number of tumors of different size and location, massed together in the different layers of the uterus, and forming a tumor of the size of the uterus in the seventh month of pregnancy.

The operation was performed in the same manner that I am about to describe in the following cases, and recovery took place without any untoward symptom, except that about two months after the operation the elastic ligature by which the stump of the uterus had been secured, passed away through the external os. The lady is now in flourishing health. About nine months ago she passed a considerable quantity of blood with her urine for three or four days, with some feeling of pain in her back and a general sensation as if she had her period. The urine afterward became normal, and, so far as I know, no such hemorrhage has recurred. She pretends to enjoy sexual intercourse without impairment as compared with her healthy period of married life before the operation. Lately she has become rather stout.

CASE IV.—In January of this year Miss W., thirty-three years of age, consulted me for an abdominal tumor which had been noticed for the past three years, but only lately attention had been called to it, by its more rapid growth and large size having been the source of disturbances. The whole abdomen seemed to be occupied by a resistant, rather smooth tumor reaching from the os pubis high up to the epigastrium and the free border of the ribs. The tumor was very movable and allowed of passing the fingers partly under it toward the entrance of the small pelvis, so that on the first examination it did not seem to take its origin from a pelvic organ, though its movements were communicated to the uterus. A closer examination, however, revealed a connection with the fundus uteri and made the diagnosis of a pedunculated fibrous tumor the most probable.

In this case the operation was comparatively simple. Though it would have been possible to remove the tumor without sacrificing the internal sexual organs, I still deemed it advisable to perform the supravaginal amputation for the following reasons: First, I thought of the possibility, that from its rapid growth within the last months the tumor might have assumed a more malignant character; and secondly, several small beginning fibromata could be distinguished lower down within the walls of the uterus, which if left behind would have developed further and perhaps more rapidly.

The operation was performed in the following manner. Through an incision in the linea alba reaching from the epigastrium almost down to the symphysis pubis, the tumor was slowly and without difficulty brought before the abdominal walls, the adhesions were but very slight. Enormously dilated veins occupied the broad ligaments. Then from both sides in a horizontal direction the broad ligaments were tied in several portions between two ligatures until close to the lateral edges of the uterus, and cut across. I then passed an elastic ligature under the peritoneal covering of the cervix uteri, and tied it by means of coarse silk thread. About three cm. above this ligature the uterus was amputated and thus the whole mass removed. The tissue of the stump was then excised in the shape of a funnel, so that the mucous membrane was removed as low down as the elastic ligature permitted. At the deepest point the actual cautery was applied and a small quantity of iodoform powdered over the eschar. The funnel was then closed by a number of deep catgut sutures, between which the peritoneal covering was adjusted by superficial ones. The ends of all the ligated tissues were then cauterized, sprinkled with iodoform, and the abdomen closed by peritoneal and other sutures in the usual manner. The patient had a very rapid recovery almost without any feverish reaction, and is so far entirely well.

In a case like this the operation does not present any great difficulty, nor does it involve any particular danger if only proper care is taken not to expose the patient to an unnecessary loss of blood. I think the patient, apart from the blood that was contained within the removed parts, lost hardly more than an ounce of blood. The way of passing the elastic ligature beneath the peritoneum was intended to secure a certain amount of nutrition for the stump. The latter, in fact, did not become entirely blood-

less, but presented, during the act of being tightly closed up by stitches, some slight oozing which, however, was safely checked by the sutures.

The weight of the tumor now, after a five months preservation in alcohol, is nine and a half pounds. It is throughout all its substance a fibromyoma.

The second specimen, which I present here, was obtained by an operation of much more seriousness and difficulty. In fact, the operation was only undertaken at the urgent request of the patient.

CASE V.—Miss L., thirty-three years of age at the time of the operation, had from her twenty-sixth year suffered from abundant menstrual hemorrhage due to a tumor. She was treated with hypodermic injections of ergot, some of which, she states, were injected into the mass of the tumor itself and gave rise to an intense peritonitis which lasted for six weeks. No relief followed. The tumor gradually increased in size. Within the last months her suffering had become most intense. She had constant pain, was hardly able to walk, her digestion became impaired, and she lost rapidly in strength and flesh. The external examination revealed the presence of hard, lumpy, irregular masses in the lower part of the abdomen, which, on the left side, extended close to the border of the ribs. A large portion of the tumor could be felt from the vagina reaching far down into the small pelvis, displacing the os uteri toward the symphysis pubis and pushing it in an upward direction. Everything seemed fixed and immovable. On the 11th of January the tumor was removed. Gradually the numerous adhesions with the anterior abdominal wall, the omentum, and intestines were removed. There existed venous vessels in such quantity and of such enormous size as I have never seen before, and requiring careful ligation. A great difficulty was experienced in shelling out that part of the tumor, which on both sides had grown under the peritoneum between the layers of the broad ligaments. At last the cervix uteri was reached, an elastic ligature applied, and the mass removed. The rather free hemorrhage from the extensive raw surface, which corresponded to the subserous attachment of the tumor, was at last checked by ligatures, sutures, and the actual cautery. A careful toilet of the peritoneum followed and the abdomen was closed. Repeatedly during the operation the patient had very alarming attacks of heart-weakness, which obliged me to interrupt the operation and at one time apply artificial respiration. It seemed to me that these attacks depended on forced tractions which could not very well be avoided during the attempts to get under the immovable and fixed mass.

In this case the cervix uteri was entirely void of peritoneal covering, which had been lifted from it by the adjacent subserous tumor masses. The mucous membrane of the cervix was excised as in the case before, but the indication to finish the operation was so urgent that I had to desist from any further details in treating the stump of the uterus. Altogether the operation had lasted about three hours and a half. The patient had a very protracted convalescence disturbed by peritonitis and the formation of an abscess, which discharged itself spontaneously through the cervical channel. Another abscess formed itself near the anterior abdominal wall, and was opened in the line of the original incision, leaving a fistula. On the 24th of April I dilated the cervix on account of persistent offensive suppuration, and extracted quite a number of coarse silk ligatures, but was unable to find the elastic ligature, which apparently was safely encysted. Several other ligatures were removed through an incision in the abdominal wall a few centimetres to the left from the original cut, and since then the discharge has almost entirely disappeared; the patient does not suffer any more, and can be regarded, I think, as definitely convalescent. The weight of the alcohol specimen is ten pounds.

I may be permitted to add, that a patient from whom I removed the uterus for myxosarcoma about four or five years ago, and whose specimen and history were presented before this Society, is so far enjoying good health. I saw her about one year ago, and there was no evidence of any recurrence of the disease. She promised to let me know if in any way she should be troubled.

I should also like to mention a case of multiple fibromyomata of the uterus, in which my operative efforts were not followed by such good fortune:

CASE VI.—A recently married woman of about thirty years, nullipara, had been suffering similarly to the above-mentioned patients during her menstrual period. Lately, a great deal of pain and tenderness in the hypogastric region had supervened, and the patient, asking far a radical curative effort and having exhausted other treatment without success, was subjected to laparomyomotomy. Two pedunculated fibromata about the size of a small fist were found, arising from the fundus uteri; the one impacted in Douglas's space was easily recovered after ligating the pedicle. The other, to the right side of the uterus and in front of the broad ligament, had become necrotic in consequence of torsion of its pedicle, and was embedded in a sac of adherent peritoneum, which, however, could be detached without great difficulty. There was no exudation of pus or fluid. The tumor looked gray, with a greenish tint, and contained no fluid blood. I tried to tie the pedicle within its living part, but a small necrotic portion remained beyond the ligature. A third myoma, about the size of a small hen's egg, was broadly inserted on the anterior wall of the uterus right above the cervix, and for its removal and shelling out a thin layer of uterine tissue had to be severed. There was considerable capillary oozing from the bed of the tumor, and the efforts to check the hemorrhage by Paquelin's cautery, stitches, and ligation, made the operation prolonged. Death occurred on the third day, apparently from septic peritonitis. No autopsy was conceded.

I presume the infection may have started from the place where the necrotic fibroma was embedded. Here, perhaps, infectious germs existed, which, though made harmless for the time by the adherent peritoneum, were set free by the operation, and found outside of their prison very favorable chances for their deleterious action. The torsion of the pedicle illustrated very beautifully the way in which such tumors at last might be deprived of their blood supply, and finally undergo retrogressive metamorphosis.

With reference to the supravaginal amputation of the uterus, I should like to mention, in a few words, the different ways in which the uterine stump is secured and treated. Thus, as previously in ovariotomy, the extra- and intra-peritoneal method stand against each other, and on both sides equally good results are obtained. Schröder and Martin, in their very extensive practice, trust to tight suture of the stump after funnel-like excision. Schröder first advised étage suture. They do not use the permanent elastic ligature, and are strong advocates of the intraperitoneal treatment. So is Olshausen, who uses the elastic ligature, Rose, and others; while extraperitoneal treatment is given preference by Hegar,

Kaltenbach, Péan, and others, as involving less risk in reference to septic poisoning, and leaving the stump accessible in case of secondary hemorrhage.

There exists, perhaps, no more striking illustration of the safety of aseptic proceeding than the fact that such large masses of tissue or the ligated stump of the fibromyomatous uterus can be left in the abdominal cavity, deprived of blood supply, without undergoing decomposition, and giving rise to infection. On the other hand, it cannot be doubted that a certain amount of risk will always be attached to their way of acting, and that those methods which secure nutrition to the stump with intraperitoneal treatment will have to be regarded as the most proper ones. That good results can be achieved by the extraperitoneal method nobody will doubt, and repeatedly successful cases of it have been reported to this Society. I am, however, convinced that, by-and-by, just as it has been with ovariotomy, the intraperitoneal treatment will be more and more adopted. I should certainly give preference to Schröder's method of securing the stump by tight étage sutures, above all others, if with reference to hemorrhage I should regard it as free from all danger. It may be safer in the hands of operators, who are working on such a large scale as Schröder and Martin do. I must myself concede, that heretofore only the apprehension of a possible hemorrhage has prevented me from following them. In order to combine the advantages which the elastic ligature offers in regard to hemorrhage, with those offered by suturing the stump, I have tried, in one of the above cases, to apply the ligature beneath the peritoneum, so that a certain amount of blood can be furnished to the stump through its peritoneal covering, which, of course, must be detached as little as possible above the ligature. Further observations will have to be made, in order to ascertain whether this procedure will always yield as good results as it has in my case as above recorded.

One mechanical contrivance I would mention, and of which I like to make use in all abdominal operations wherever practicable, is Thiersch's ligature spindles, with or without holder, which I have presented at a former meeting of this Society. They are, indeed, very handy, and allow of securing pedicles and fleshy adhesions with more constricting force, than can be exerted in the ordinary way of tying.

The abdominal incision ought to be long enough to allow of an easy access to the operating field. I certainly prefer too large an incision, to one which obliges the operator to work in the dark, or to do uncertain manipulation. For this latter reason, often one of the principal dangers in these operations is incurred, namely, unnecessary and abundant loss of blood, simply because the operator does not in time and with easiness get at the bleeding point. If one has seen those enormously dilated veins in these tumors, one must be convinced that bleeding from such a vessel for a very short time must be sufficient to endanger life. For this reason, also, every undue tearing force ought to be avoided, and it is safer and more wise to work slowly and cautiously, than to hurry at unnecessary hazard. The length of the abdominal incision adds almost nothing to the danger and gravity of the operation, and incisions of fifteen or more inches will heal without difficulty. To obviate loss of warmth from extensive denudation of intestine, the use of large, flat sponges dipped in a weak aseptic fluid seems to me the most commendable.

MEDICAL PROGRESS.

WATER PURIFICATION.—At a recent meeting of the Institution of Civil Engineers, the paper read was on "Water Purification: Its Biological and Chemical Basis," by Percy F. Frankland. Ph.D., B.Sc. F.C.S. The earliest attempts to purify water dealt simply with the removal of visible suspended particles; but later, chemists turned their attention to the matters present in solution in water. Since the advance of the germ theory of disease, and the known fact that living organisms were the cause of some, and probably of all, zymotic diseases, the demand for a test which should recognize the absence or presence of microörganisms in water had become imperative. It was, however, only during the last few years that any such test had been set forth, and this was owing to Dr. Koch, of Berlin. By this means the only great step which had been made since the last Rivers Pollution Commission had been achieved. It had been supposed that most filtering materials offered little or no barrier to microörganisms; but it is now known that many substances have this power to a greater or less degree. It has also been found that, in order to continue their efficiency, frequent renewal of the filtering material is necessary.

Vegetable carbon employed in the form of charcoal or coke was found to occupy a high place as a biological filter, although previously, owing to its chemical inactivity, it had been disregarded. Being an inexpensive material, and easily renewed, it is destined to be of great service in the purification of water. Experiments were also made by the agitation of water with solid particles. It was found that very porous substances, like coke, animal and vegetable charcoal, were highly efficient in removing organized matter from water when the latter came in contact with them in this manner. Also, it was found that the well-known precipitation process, introduced by Dr. Clark, for softening water with lime, had a most marked effect in removing microörganisms from water. In the case of water softened by this process, it was found that a reduction of 98 per cent. in the number of microörganisms was effected, the chemical improvement being comparatively insignificant.

Water which had been subjected to an exhaustive process of natural filtration had been found to be almost free from microörganisms. Thus, the deep well water obtained from the chalk near London contained as few as eight organisms per cubic centimetre, whereas samples of river water from the Thames, Lea, and Wey had been known to contain as many thousands. The water supplied to London had been regularly tested during the last fifteen months, and most important and valuable information had been obtained as to the efficiency of the processes to which the water companies subjected the water supplied by them in removing microörganisms, the average reduction during the last four months of the past year having been 97.9 per cent. for the Thames

and 93.7 per cent. for the Lea. The biological testing of waters is of especial value to waterworks engineers, for they now have a means of ascertaining with exactitude the working condition of filter beds, instead of following the empirical methods generally in use.

ANTISEPSIS OF THE DIGESTIVE TRACT.—In a recent work on gastric dilatation, LEGENDRE presents a discussion of intestinal antisepsis, which may be summarized as follows:

A medicament administered for the disinfection of the digestive tract should be but slightly, if at all, absorbable.

It should be capable of administration in efficient doses without producing toxic effects, and must exert its specific effect in the long transit from mouth to anus.

The salicylate of bismuth, recommended by Vulpian, is of interest in this connection. The salt is gradually decomposed in its passage through the intestine into a black sulphide, with a simultaneous slow liberation of salicylic acid, which is subsequently eliminated by the kidneys.

Iodoform, exhibited for the same purpose, is slightly decomposed, traces of iodine being found in the urine, and the major portion of the drug passing out unchanged with the feces.

Naphthaline is similarly excreted, the urine being slightly tinged, though the greater part of the dose is unchanged by digestive fermentative action. Bouchard strongly recommends the following mixture for typhoid and other similar putrefactive conditions of the intestinal contents:

Glycerine	200 parts.
Pulverized charcoal . . .	100 "
Naphthaline	5 "
Iodoform	1 part.

A teaspoonful of the mixture is given in a wineglassful of water every two hours.

In gastric trouble, with diarrhœa of less intensity than in typhoid, the naphthaline may with advantage be administered alone, as follows:

Naphthaline,	
Pulverized sugar	āā ℥iv.
Oil of bergamot	gt. j.

This is mixed and divided into twenty powders, one of which is given every hour.

The last two methods of treatment are more efficacious than the administration of salicylate of bismuth, or of water saturated with carbon disulphide. The relative value of these various methods of intestinal disinfection has been experimentally determined by intravenous injections of the diarrhœic flux obtained before and after each method of treatment respectively.—*Revue de Thérapeutique*, May 15, 1886.

COMMENCEMENT OF DROPSY AS A POINT IN DIAGNOSIS.—DR. B. W. RICHARDSON speaks as follows in *The Asclepiad* for April 25, 1886:

Apropos of the subject of dropsy, there has seemed to me to be one very good mechanical indication for distinguishing whether dropsical effusion springs from disease of the heart, or from disease of the liver, or the first local seat of the effusion. If dropsical effusion begins in the lower extremities, and proceeds to the abdomen, the evidence is fairly sound that the obstruction is direct from the heart, and that the liver is free, the stagnation and transudation being due to a failure of the return column of blood from the extreme parts. If, on the other hand, the accumulation of fluid begins in the abdomen, and extends to the lower limbs, the evidence is equally good that the arrest of the circulation is in the hepatic system. This distinction may be of moment sometimes in deciding on the question of tapping. In a case where the dropsy occurred first in the extremities, and afterward in the abdomen, it would be good practice to delay the tapping until the effect of removing fluid by puncture of the lower limbs had been carried out. But in a case where the effusion is, primarily, into the peritoneum, and afterward into the extremities, tapping the abdomen would have a prior place to puncture. The rule would run, in cases where there are ascites and anasarca: Tap for primary ascites, puncture for primary anasarca. I refer, of course, in this note to chronic ascites and chronic anasarca uncomplicated; not to special dropsies of local origin, like ovarian cysts, and not to dropsy from renal disease.

PEPTONURIA.—From an elaborate paper, containing the results of 810 urinalyses, by PACANOWSKI, the following points of clinical interest may be extracted: Acute peptonuria was found in 25 out of 36 cases of abdominal typhus. Chronic peptonuria existed in 11 out of 25 cases of phthisis pulmonalis. It was chiefly to be found in the beginning of the disease, and was only exceptionally present after the origin of cavities.

In six cases of carcinoma (œsophageal, gastric and intestinal) peptone was constantly present in the urine. —*Centralb. f. klin. Med.*, May 15, 1886.

LOCAL APPLICATIONS FOR USE IN NEURALGIAS.—The *Revue de Thérapeutique* for May 15, 1886, gives the following formulæ.

Intercostal neuralgia may be greatly relieved by daily gentle inunctions with a portion, of about the bulk of a pea, of a pomade thus constituted:

Morphine chloride,	
Veratrine	gr. iss.
Cold cream	℥iv.

In lumbago or painful contraction of the muscles of the back a liniment containing one part of tincture of capsicum in six parts of olive oil, is advantageously applicable. If its infriction be painful, it may be applied on flannel.

INCIPIENT BALDNESS.—In commencing alopecia, VIGIER advises the use of the following formula, in which the proportions are given by weight:

Alcohol (80°)	℥xx.
Camphorated alcohol,	
Rum,	
Tincture of cantharides,	
Glycerine	āā ℥iv.
Essence of sautal, wintergreen, laurel roses	āā gtt. v.
Muriate of pilocarpine . . .	gr. viij.

The mixture is gently rubbed on the scalp once daily. —*Revue de Thérapeutique*, May 15, 1886.

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SATURDAY, JUNE 12, 1886.

THE DISINFECTION OF RAGS.

THE controversy which has been going on between the health officers of certain ports and the importers of foreign rags brings to mind the old saying about sanitarians and men of business, viz., that the first would have everything considered dangerous until it is proved to be harmless, while the second would consider everything harmless until it had been proved to cause evil. The wise course in this, as in many other things, is between the two extremes.

Judging by the experience of the past, there is very little danger of the introduction of cholera into this country from the importation of baled rags even when these are collected in countries in which cholera has recently prevailed. The cholera germ does not appear to possess such vitality as to be able to endure for a month or more the conditions in which it must be placed in baled rags; and, as several months must almost always elapse between the collection of rags in a foreign country and their arrival at our ports, it is not surprising that thus far there is no known instance of the introduction of cholera in the United States by this channel. It is theoretically possible that rags wet with cholera discharges, and kept moist, might, by unusual promptitude in shipment, reach this coast in time to be dangerous, but this contingency is a remote one.

It would seem, therefore, that the importers of rags have a right to complain when baled rags coming from a country in which there has been no cholera for several months are forbidden admission except under conditions as to disinfection which largely increase their cost, and especially when such restrictions are enforeed only at certain ports, while at other ports the same rags are admitted freely. Under such circumstances they claim that little or no protection is afforded the public health, and that the only practical result is specially to tax certain importers, and to drive away trade from the ports where the health officers make unreasonable demands. It is a particularly unfortunate feature of the present controversy that the form of disinfection insisted on by certain health officials is practically that controlled by a patent, the owners of which have a monopoly and charge six or seven times the actual cost of doing the work.

On the other hand, the health officer is bound to resist the pressure of commercial interests of his own port, when these urge him to a course which he believes to be dangerous to the health of the country at large, and he has to consider not only the real danger, but, to some extent, popular opinion as to the danger, since, if he do not, he runs a risk of bringing about more or less suspension of trade with his own port on the part of the interior of the country.

The conclusions of the New York Chamber of Commerce, which has been for some time engaged in investigating the rag disinfection controversy at that port, are that the health officer of the port of New York has been injudiciously severe in his requirements, and the Chamber lays great stress on the desirability that quarantine regulations affecting merchandise should be the same at all ports, and that therefore the whole business of quarantine should be managed by the General Government. Admitting this to be true, as, with some minor limitations, we are disposed to do, it is nevertheless very certain that at present there is not the slightest probability of Congressional action in this direction.

The matter must be settled by State and Municipal Governments and their officials; but these last, if they are wise, will not lay themselves open to the suspicion that they are actuated rather by a desire to promote the interests of the stockholders of a particular disinfecting company than a desire for the public good. The danger should be urgent and clearly demonstrable to justify their taking such a risk.

Where there is evidence sufficient to justify an impartial man that baled rags arriving at one of our ports have come from a cholera-infected port, or have been recently collected in a cholera-infected country, public opinion will justify a health officer in almost any precautions he may see fit to take, even to the total exclusion of the suspected bales; but in the absence of such evidence, and when the rags are accompanied by a positive certificate from a consul as to the circumstances under which the rags have been gathered and shipped, indicating the absence of cholera in such localities, we do not be-

lieve that it is justifiable to compel the importer to submit to the cost of a special patented disinfection process.

PASTEUR'S METHOD OF TREATING HYDROPHOBIA.

IN connection with the paper of Dulles on Pasteur's recent communications in regard to rabies, it is interesting to note that FRISCH has just made an elaborate report to the Society of Physicians of Vienna giving the result of his personal investigation of Pasteur's method, made in Paris at Pasteur's laboratory, as the selected representative of the Austrian medical men. This report seems to have been misunderstood by some medical journals; they have reproduced the version of it given in a number of French papers, which speak of it as expressing unreserved praise of the method of Pasteur, and as expressing the opinion that it should be adopted by all the physicians of Austria. In concluding his report Frisch actually says: "It is not yet possible to pronounce with certainty upon the value of the preventive treatment . . . but there is nothing to prevent a repetition of M. Pasteur's experiments."

From an interesting account of the meeting at which this report was made, which appears in the *Deutsche medicinische Wochenschrift*, for April 29, 1886, we find that Frisch expresses some very serious objections to the acceptance of Pasteur's method. He says: "It is easy to show what kind of experiments should have been performed before the experiment on human beings was indicated. A number of animals should have been subjected to the bite of a rabid animal. Part of the bitten animals should receive the preventive inoculations, the others not. If the method is effective the first animals would remain healthy, the others would die of rabies. Still, even this procedure might not be just the right one, since infection might not take place in all the animals with equal certainty. But there is a method which would preclude all doubt. Pasteur has manufactured a rabic virus which leaves nothing to be desired in the activity and certainty of its action. This rabic virus must be injected into animals, and, as in the first case, it must be shown that the animals are thereby uniformly killed. It must be shown, on the other hand, that when 'preventive inoculations' are afterward practised upon animals which have been inoculated with the deadly virus, rabies does not appear. This is an experiment analogous to the case of human beings who have been bitten. But we seek in vain to learn that such experiments have been undertaken. And can we understand what a contradiction is involved in such experiments? If a human being infected with the deadly rabic virus can be cured by supplementary inoculations, if an animal which has been experimentally infected with the same sort of virus can be preserved from rabies by a similar procedure, then the whole procedure of actual preventive inoculations is useless and senseless. For, why is it necessary to administer the rabic virus at first in minimal and then in gradually increasing doses, since the cure of rabies in human beings and lower animals by such inoculations would show that it is possible to give the strongest and most murderous dose at first, and one need only administer the weak attenuations of the virus after this."

It will be seen that Frisch points out the fact that none of Pasteur's experiments for the protection of lower animals from the effects of inoculation of rabic virus are analogous to the application of his method to mankind. In the former, animals were prepared for the inoculation of a presumably effective virus by previous inoculations with a presumably weaker one. In the latter case the presumably effective virus is inoculated a considerable time before the inoculation with the weakest virus. Notwithstanding this, if the application of Pasteur's method to mankind demonstrated that it is useful, we might well overlook the illogical process of reasoning by which he has arrived at it, and acknowledge that Pasteur has made a lucky hit which a more logical worker might have missed. As yet, however, it must be admitted that there is no conclusive evidence that Pasteur has rescued a single patient from the effects of an inoculation with rabic virus, which without his intervention would have been followed by hydrophobia, while the cable this week brings us news of another (the eighth) death, while under his care, this being a patient upon whom he began his inoculations within fourteen days after the bite was received. In view of these facts, it is not hypercritical to ask for positive proof of the correctness of a claim which is not logically established by the argument of the claimant.

THE TEMPERATURE OF PERIPHERAL PARTS.

OUR knowledge of the relations of the peripheral temperature of the body has been considerably increased by the observations of SCHWARZ, which appear in *Deutsches Archiv f. klin. Med.*, Bd. xxxviii. A thermometer of special construction was used, and the space between the first and second toes was chosen as the most suitable locality. The observations were continuous, extending over many hours, and the records taken every fifteen minutes. The axillary temperature was taken as the standard of comparison.

It was ascertained, in the first place, that in health the difference between the temperature of the fingers and toes was trivial and the variations in the corresponding regions of the body very slight. As a result of the continuous mode of observation, the interesting fact was elicited that in healthy per-

sons two stages could generally be recognized: a period in which the temperature was very much lower than that of the axilla and a period in which it almost or fully equalled it. The period of low and variable temperature, which he terms amphibolic, may last from fifteen minutes to five hours, and in some instances is not present, but the axillary and interdigital readings may be the same from the outset. In twelve observations the average duration of this amphibolic period was one and a quarter hours. Reckoning as normal that stage in which the highest temperature is reached and maintained continuously, we may say that the peripheral temperature is not more than 0.9° to 1.8° F. below that of the axilla. The reason of this preliminary depression is not very clear, but it is possibly associated with vasomotor disturbance.

In afebrile patients the observations did not lead to any very striking results. The higher temperature in paralyzed limbs is well known, and in a case of cerebral hemiplegia the maximum temperature was reached very slowly, not until five and a quarter hours, and then was nearly 7.2° F. higher on the affected than on the sound side. An interesting contrast to this was afforded by a case of hysterical hemiplegia in which the difference between the sides was very slight and the amphibolic stage scarcely existed. In the paraplegia of myelitis and in tabes the peripheral temperature was continuously low, as a rule, and there were great variations between the sides.

In fever the initial stage of low temperature is short, a higher grade is quickly reached, and there are greater variations than in health. A large number of records were made with a view of testing the action of various antipyretics on the peripheral temperature, and it was found that in the cases, twenty-five in number, in which the thermometer between the toes registered 94.3° F. at the time of administration, there was invariably a rise in the temperature, while in thirty-two cases with a peripheral temperature above 96.8° F. this never occurred.

While in health the peripheral temperature never exceeds that of the axilla and in febrile patients it very rarely does so, Schwarz has called attention to an interesting condition which he terms peripheral fever, in which, with a normal axillary temperature, the thermometer at the periphery may register several degrees above the normal. This was met with in a case of biliary cirrhosis with intense jaundice in which the axillary temperature was 100.4° F., and the peripheral, as measured between the toes, from 104° to 105.8° F. The measurements extended over from five to seven hours during several days. The patient complained of a severe sense of burning and great heat in the extremities, and could not bear heavy bedclothes though the season was winter.

A practical lesson to be derived from these observations relates to the time which must be allowed in testing the peripheral temperature. The low records of Davy and others were evidently due to the neglect of this precaution. To be of any value, the preliminary period of depression, which is variable in different individuals, must be taken into account.

FATAL RESULTS FROM "SPLITTING THE CERVIX."

DIVIDING the cervix at the external, or at the internal os, or in the intervening portion, though not long since a comparatively frequent operation for dysmenorrhœa or sterility, is now very rarely done. Most operators now turn to dilators for the treatment of cases where incision was formerly done; one wing of the army of gynecologists still fights under the same banner of mechanical uterine pathology, only in place of hysterotomes, its enthusiastic soldiers use dilators. Possibly it is only a question of time when many of the dilators will be placed in the grave beside the hysterotomes, if the teaching of men like Duncan, Schultze, and Williams prevails, and the mechanical theory of uterine disease is cast aside.

However this may be, we have been somewhat astonished to know of the mortality which Sims had from this operation. Pajot states, in a recent lecture, that he knew of at least four deaths of women upon whom Sims had performed his operation of division of the cervix, and he believes that other similar accidents happened to him. In the light of these facts, the profession is to be congratulated upon the fact that the operation has fallen into disuse.

SOCIETY PROCEEDINGS.

THE AMERICAN LARYNGOLOGICAL ASSOCIATION.

Eighth Annual Session, held in the Hall of the College of Physicians of Philadelphia, May 27, 28, and 29, 1886.

THURSDAY, MAY 27—FIRST DAY.

AFTERNOON SESSION.

DR. F. H. HOOPER, of Boston, presented a paper

CONCERNING THE POSITIONS OF PARALYZED VOCAL BANDS.

The paper was, in part, based on experimental work done in the physiological laboratory of the Harvard Medical School, and in part theoretical. Specimens and drawings, illustrating certain points, were shown. The reader stated that it was his purpose to inquire into certain influences which might combine to determine the positions of paralyzed vocal bands, and to ask whether we are always justified in assuming that a given position of an immobile band is indicative of the arrested function of this or that muscle of the intrinsic laryngeal groups. The speaker thought that a vocal band might assume different positions in cases of complete paralysis of the recurrent nerve, and he passed

in review certain anatomical factors and physical causes which he thought might contribute toward producing the position which might happen to be present when seen reflected in the laryngeal mirror.

Dr. Hooper concluded by saying that in his judgment the larynx is such a complicated organ, subject to such changes at different stages of life, of a shape so different in different individuals, its nerve supply so great, and the arrangement of its muscles so liable to anomalies, that as surely as one attempts to formulate theories on the positions alone of paralyzed vocal bands —positions which may be controlled by intrinsic muscles of the larynx that are not paralyzed, as well as by those that are—just so surely will one be led unconsciously into erroneous inferences.

DR. J. SOLIS COHEN, of Philadelphia, then related

THE FURTHER HISTORY OF A CASE OF PARALYSIS OF THE POSTERIOR CRICO-ARYTENOID MUSCLES PRESENTED AT THE FIRST MEETING OF THE ASSOCIATION IN 1879, WITH A REPORT OF THE AUTOPSY AND THE EXHIBITION OF A SPECIMEN.

The history of the case was briefly given as follows. A man, aged forty-eight years, was brought to the writer with a history that for two years he had suffered from occasional spasm of the larynx so that on at least two occasions he had fallen unconscious in the street. When a boy he had suppurative inflammation of the left ear, and ever since then the introduction of the finger into the ear would produce spasm of the glottis. Examination with the mirror showed what was then considered to be the picture of paralysis of the left posterior crico-arytenoid muscle. The examination produced spasm on the other side. The patient was directed to carry with him nitrite of amyl, to be used when the attacks came on. This he did for a while with good results. He subsequently returned with the statement that the day before he had had a severe spasm from tickling in the ear, and while illustrating this he had a violent attack in the office, requiring the use of chloroform to relieve it. The next day tracheotomy was performed. No other lesion could be found at that time. The spasms were still produced by irritation of the ear. Five or six weeks later he began to have paralysis of the right vocal band. The picture then became extreme, and after this time the larynx was never seen in any other position. The man gradually became blind and developed symptoms of locomotor ataxia. During the four years preceding his death he continued to wear the tube. He was able to speak well without occluding the tube. With the condition of abductor paralysis, there was apparently a condition of spasm of the adductors.

At the autopsy all the parts that could be available in the examination were removed and submited to Dr. William Osler for examination. No abnormal appearance was discovered in the brain. The optic nerve was small, grayish-white, and atrophied. Many microscopical sections were made of various parts. The left recurrent nerve seemed natural, though it will be remembered that the left cord was the first affected. The right recurrent showed a distinct line of demarcation between the healthy and the unhealthy fibres. Instead of the right muscle being atrophied, it was the left muscle which had undergone wasting.

DR. FRANK DONALDSON, JR., of Baltimore, then read a paper entitled

THE FUNCTIONS OF THE RECURRENT LARYNGEAL NERVE FROM AN EXPERIMENTAL STUDY IN JOHNS HOPKINS UNIVERSITY.

The speaker first referred to the fact that all the muscles with the exception of the crico-thyroid are supplied by the recurrent laryngeal nerve. The nerve, therefore, contains fibres controlling both phonation and respiration, acting both upon the abductor and upon the adductor muscles. He then referred to the experiments performed by Dr. Hooper, of Boston, the results of which had been presented at the last meeting of the Association. Dr. Hooper had found that the constrictors ceased to act when consciousness was suspended by the action of ether, and that abduction with dilatation of glottis was obtained by stimulating the recurrent laryngeal nerve when consciousness was suspended. Dr. Donaldson's experiments were made to test the correctness of these results. In five experiments under states of deep narcosis, slight narcosis and almost complete consciousness, he invariably got adduction of the vocal bands under the application of stimuli with the induction coil at ten. In one case in which consciousness had been suspended, under slight stimulation, abduction was produced, while under strong stimulation adduction was obtained. It was then found that abduction was always produced when weak currents were employed, but adduction was obtained when stronger stimuli were used. He, therefore, concluded that the constrictors did not cease to act during profound narcosis or in suspension of consciousness from any cause, and that abduction is not always obtained when consciousness is suspended; it is with weak currents that abduction is produced, but as the stimulation is increased adduction takes place. (Dr. Donaldson's paper will appear in full in *The American Journal of the Medical Sciences* for July, 1886.)

DR. F. H. HOOPER, of Boston, said that personally he felt very much indebted to Dr. Donaldson for having taken up this work, for it is only by getting many interested in the subject, and bringing forth their results and comparing and criticising them that we can ever hope to arrive at the truth. Dr. Donaldson had prepared his paper in the friendliest spirit toward him, but it would have been more gratifying if he had read the speaker's paper on "The Respiratory Function of the Human Larynx" with closer attention and then quoted it correctly and accurately. He objected very strongly to the wholesale manner in which he had been misrepresented, yet he recognized that it was unintentional on the part of Dr. Donaldson, who very oddly got the idea from reading his paper that *none* of Dr. Hooper's experiments had been verified. If such had been the case, the paper would never have been presented to this Association. Dr. Donaldson had laid great stress upon what he called the speaker's *theory of the innervation of the larynx*. He was not aware before that he had a theory on the subject. In his paper of last year he detailed the results obtained from certain experiments, and, in passing, offered, more as a suggestion than anything else, that they might be explained in a certain way. Dr. Donaldson had expressed agreement with him upon one point in which he least expected support, and has

disagreed with him upon points that admit of no doubt, and are so firmly established by repeated experiments, that he is convinced that the reason why Dr. Donaldson failed to confirm his observations was due to some error in Dr. Donaldson's method of experimentation. In regard to another small oversight, Dr. Donaldson had declared that he said that he had observed the same effects with morphine, chloral, and chloroform as with ether. Dr. Hooper had never made such a statement.

DR. F. I. KNIGHT, of Boston, thought we must bear in mind that these experiments have been performed upon lower animals and not upon man and even if a certain unanimity of opinion is reached, we shall have to adapt these results to the case of man by clinical and pathological observation. It does not follow that the same mechanical action takes place in the opening and closure of the glottis in man as in the lower animals. Although stimulation of the transversus muscle produces approximation of the cord, still, in considering the articulation of the cartilages it had always seemed to him that there must be some other factor, and that the lateral muscle is stimulated at the same time. In regard to abductor paralysis, he held that the whole question should be decided upon the position of the vocal process without any reference to the cord. If the vocal process is found in the median line, it is fair to assume that for some reason the function of the abductor muscle had been abolished. A good deal of difficulty has been experienced by observers in determining when the vocal process is in the median line. To him the crucial test is that of phonation; if you get a vertical glottis on phonation without any movement of the suspected cord, you have a sure case. With reference to the question of spasm, he considered it absurd for any one to consider these cases of what we call abductor paralysis to be cases of spasm. Who ever heard of a muscular spasm lasting for years without any relaxation? He did not refer, of course, to what is called by surgeons muscular contraction, but to real spasm, which, in the cases he had seen, had been more or less intermittent.

DR. S. SOLIS COHEN, of Philadelphia, in referring to the question of spasm, said that he could show several cases in which there was constant spasm as the result of an irritative lesion. In one case of injury of the spine, resulting in pressure on the cord, there was spasm of the extensor muscles on one side, and of the flexors on the other. He thought the point not well taken. It is important to distinguish between the automatic (respiration), the reflex (cough), and the psychic (phonation) functions of the larynx. A well-known physiological law is that the resistance which a nerve offers to the transmission of nervous force is inversely as the use made of the muscle which it innervates. No muscle is used more than the abductors of the larynx. The resistance of their nerves would, *a priori*, then be supposed to be the least, and Dr. Donaldson's experiments show that they respond to a weak stimulus. The phonatory muscles are not so frequently used, the resistance of their nerves, though low, is higher than that of the respiratory fibres. The respiratory impulses traverse the nerve continuously, and additional stimulation, whether reflex (irritation of the superior laryngeal), producing cough, or psychic, for purposes of phonation, produces adduction of the vocal bands. There is a local centre in the medulla, the vaso-accessorius nucleus, for all the laryngeal muscles. Moderate stimulus from the respiratory centre opens the glottis for respiration; increased stimulus opens it further; a greater increase, say by reflex irritation, closes it for cough; an impulse from the cerebral cortex, the centre of voluntary motion, closes it for speech.

Dr. F. I. KNIGHT, of Boston, presented a paper on

LARYNGEAL VERTIGO.

A number of cases of what is called laryngeal vertigo have been reported by various authors. The distinctive feature of these cases is that attacks of coughing are followed by giddiness and momentary loss of consciousness. This latter symptom was present in all but two cases. While it is probable that attacks of dizziness after coughing are not rare, yet but little attention has been paid to the subject. The writer had been able to find fourteen published cases; to these he would add two coming under his own observation. The first case was that of a man aged forty-two years, who had a chronic bronchitis of one year's duration. He had had rheumatism when fifteen years of age, and on one occasion had suffered with insomnia. No morbid condition of the heart was detected. The chest was filled with sonorous and sibilant râles. Under the use of iodide of potassium there was gradual improvement. After a day of worry and fatigue, he had a cough, followed by loss of consciousness. There was no evidence of spasm of the glottis, and no convulsive movements. He never had another attack of loss of consciousness.

The second case was that of a lady forty-seven years of age, who had previously been the subject of attacks of dizziness, the result of indigestion. She had suffered with winter cough for a number of years. During one attack of coughing she lost consciousness. During this time there was congestion of the face.

The following points were obtained from a study of the reported cases: All the cases, with the exception of one, have been in males. The average age at which the attack had occurred had been forty-seven and one-half years. The cough, which induced the attack, was slight in six cases, spasmodic in two, and severe in three. Momentary loss of consciousness occurred in fourteen cases. In one case the patient fell, but declared that he had not lost consciousness. Dizziness is mentioned in eleven cases; in four cases there seems to have been decided evidence of laryngeal spasm; in one this was doubtful; in four cases there was marked congestion of the head and face; in two the patients were pale. Convulsive movements of the limbs occurred in three cases; in one, the movements affected the face and head, and in another the face. In these cases there was no biting of the tongue, no frothing of the mouth, and no involuntary micturition. The speaker objected to the term laryngeal vertigo. There probably is no real vertigo in these cases in the sense of the vertigo seen in aural vertigo, there is simply giddiness or lightness of the head. These cases cannot be considered to be cases of petit mal. The cause of the attacks is disturbance of the cerebral circulation, perhaps due to the compression of the large bloodvessels of the chest and perhaps the heart. Even without this, the effect of rapid breathing is well known. This causes marked cerebral disturbance.

A paper by DR. FRANK DONALDSON, of Baltimore, entitled, *A Case of Congenital Defect of the Epiglottis, Illustrating its Function*, was read by title.

DR. D. BRYSON DELAVAN, of New York, then read a paper on

LINGUAL TUBERCULOSIS.

He had seen six cases of this affection. In the reports of twenty-four cases of tuberculosis of the tongue, in all but one the subjects were males. In twelve cases the lesion was anterior, in seven upon the side, and in one at the base of the tongue. In four the situation was not stated. The disease was primary in nine cases; secondary in seven cases. In the remaining cases it was not stated whether the disease was primary or secondary. The longest duration of a case of primary tuberculosis was two and one-half years, the shortest ten weeks. The age of the oldest person affected was seventy years, and of the youngest, not under twelve.

The first case coming under the speaker's observation was H., aged thirty-five, a laborer. His family history and previous history were good. The man was remarkably robust. Six weeks before coming under observation, he experienced pain at the base of the tongue. This pain became excessive, and at the end of two weeks he was obliged to give up work. On examination a fissured and ulcerated surface was found at the base of the tongue on the right side. This lesion was indurated. In the centre was a large ulcer with large flabby granulations. On the right side, externally under the angle of the jaw, was a hard tumor about the size of a chestnut. There was slight fetor of the breath. The diagnosis was uncertain. The pain was so severe as to prevent sleep. An application of a four per cent. solution of cocaine checked the pain for five hours. The patient's condition became so serious that he was removed to the New York Hospital, where under the idea that the disease was epitheliomatous, the complete removal of the tongue was made. The patient recovered rapidly from the operation and there was marked improvement in the general condition. It has been impossible to find any evidence of disease of the lungs or other internal organs. On microscopic examination all the parts presented abundant tuberculous material. A point of interest in this case is, that this is the first case on record in which such an operation has been followed by an apparent cure. Of course, sufficient time has not elapsed to speak positively as to the ultimate result. The operation was done in the latter part of December, 1885.

In a second case of tubercle of the tongue, the patient P. O., aged twenty-seven, gave no history of hereditary tuberculosis. Began to run down two years ago and developed chronic laryngitis. When examined he presented evidence of advanced tubercular disease. The larynx was ulcerated. There was also a lesion on the right tonsil and one on the left. The pain on swallowing was severe. Iodoform gave but little relief. Temporary ease was given by cocaine. An application of lactic acid was made to the right tonsil, and two days later it was much improved. This was repeated and the improvement continued for two weeks, when the patient began to run down and died. The bacillus tuberculosis was not found, but the man had evident pulmonary tuberculosis.

The third case was that of a man aged forty-three. Family history good. No history of specific disease. He was well until ten months ago when his strength began to fail and cough came on. Two months ago the throat began to grow sore, and within the last two weeks this has increased. Careful examination of the chest failed to reveal any evidence of disease. Examination of the fauces showed ulceration extending out along the upper border of the uvula. Later the signs of pulmonary involvement became evident. Applications of lactic acid gave only temporary benefit. In this case, the disease has extended along the gum.

Three other cases of somewhat similar character were reported.

DR. E. L. SHURLY, of Detroit, held that in these cases of so-called primary tuberculosis mistakes of diagnosis are sometimes made. We do not diagnose the presence of other lesions when we should, and sometimes we diagnose as tubercle that which is not. He was not sure that the bacillus can be regarded as an absolute evidence that the disease is tubercle in one case, which he considered to be a case of tubercle of the larynx and in which the bacillus tuberculosis was found, the patient recovered and is still living, five or six years later. There was never any evidence of a pulmonary lesion. In another case in a woman under the most favorable conditions for the development of tuberculosis, the bacilli were found in an abscess at the base of the tongue. The patient, however, recovered, and is still well. In the treatment of this condition, the part should not be excised, but some treatment which will destroy the part should be instituted.

DR. J. SOLIS COHEN said that within a few months he had had a case of what seemed to be tuberculosis of the lungs and larynx, but he had been unable to detect the bacilli. This case is notable for the entire absence of pain. On examining the urine, it was found that it contained sugar. Since then he had been in doubt whether or not this might not this might not be a case of diabetic phthisis, and this might account for the peculiar manifestations.

DR. J. O. ROE, of Rochester, had never seen a case of what seemed to be primary tuberculosis of the tongue, which he did not consider secondary. He reported two well-marked cases of primary tubercle of the pharynx. The first case was that of a lady thirty-five years of age. Six months before coming under observation, she suffered great pain in the throat. A well-marked lesion was found above the border of the epiglottis and at the base of the tongue. This improved under local treatment. Subsequently the ulceration returned, but the patient ran down in health and died. The larynx was entirely free from ulceration. The second case was a gentleman forty-five years of age, who had tuberculous ulceration of the pillars of the fauces. Later ulceration of the larynx developed. Under the persistent application of iodide of amyl continued for some time, the patient recovered and is still well, weighing 225 pounds.

FRIDAY, MAY 28.—MORNING SESSION.

At the executive session the following business was transacted:

OFFICERS ELECTED FOR 1887:

President.—Dr. E. Fletcher Ingals, of Chicago.

Vice-Presidents.—Dr. E. Carroll Morgan, of Washington, and Dr. J. N. Mackenzie, of Baltimore.

Secretary and Treasurer.—Dr. D. Bryson Delavan, of New York.

Additional Member of the Council.—Dr. F. H. Hooper, of Boston.

The following active members were elected: Dr. Benjamin F. Westbrook, of Brooklyn; Dr. Frank Donaldson, Jr., of Baltimore; Dr. Alexander Mac Coy, of Philadelphia; and Dr. J. C. Mulhall, of St. Louis.

Prof. Ranon de la Sota y Lastra, of Seville, Spain, was elected a Corresponding Fellow.

It was decided to hold the next meeting in New York, the time to be decided by the Council.

A proposition in reference to the formation of a

CONGRESS OF AMERICAN PHYSICIANS AND SURGEONS

was received and approved, and the following committee of conference was appointed: Drs. J. Solis Cohen, of Philadelphia; G. M. Lefferts, of New York; F. I. Knight, of Boston; F. H. Bosworth, of New York; and E. L. Shurly, of Detroit.

DR. E. L. SHURLY, of Detroit, related his observations on

THE USE OF SOME OF THE NEWER REMEDIES IN DISEASES OF THE UPPER AIR-PASSAGES.

He took up in detail some of the numerous new remedies proposed, and gave the results of his experience in their use.

DR. J. N. MACKENZIE, of Baltimore, said that although it had not been mentioned in the paper, he desired to state that he had found the internal use of dioxide of hydrogen of decided value in old cases of catarrh of the larynx or pharynx. He had used it in cases which have resisted almost all other forms of treatment. He employed a four per cent. solution, giving it in doses of from two to four drachms four to six times a day. This is kept up for two or three weeks. In a fair proportion of cases the discharge is arrested by this remedy alone.

DR. S. SOLIS COHEN, of Philadelphia, had used the dioxide in cases of phthisis and of laryngeal phthisis and had observed from it good effects upon the uutrition. One of the effects of the administration of this agent is a remarkable retardation of the pulse-rate. In one case this was so marked as to compel the discontinuance of the drug.

DR. WILLIAM H. DALY, of Pittsburg, then read a paper entitled

THE SIMPLEST AND MOST EFFICIENT TREATMENT OF DIPHTHERIA.

The speaker stated that he did not intend to offer a new treatment, nor one which would cure ninety or one hundred per cent. of the cases. He would not discuss the point whether diphtheria and croup are identical. His object was to describe the calomel treatment. This is an old treatment; it is mentioned in the writings of the Rev. Sydney Smith. About 1779 Sydney Smith's daughter, when six months old, was taken sick with a violent attack of croup. Dr. Hamilton, of Edinburgh, was sent for, but not being able to come, sent directions to "give the child two grains of calomel every hour and to persevere. He had never known this to fail." This was done and the child recovered.

The credit of practising and recommending this treatment in modern times belongs to Dr. William C. Ritter, of Pittsburg. Of the different forms of mercury, calomel is the best. The drug is to be given in doses of from two to five grains every one, two, or three hours. It may be given dry and washed down with a little ice water, or it may be given in water. This is to be continued until the stools become frequent and contain in them gelatinous masses of a bright green color, resembling chopped spinach. Then the interval is to be lengthened and the drug continued so as to keep up this condition of catharsis, the patient having two or three stools each day. It is better to lengthen the interval than to diminish the dose. In this way there is less liability to induce ptyalism. Very little depression follows these large and frequently repeated doses. Ptyalism does not often occur. Under this treatment the membrane exfoliates and reforms, if at all, with less readiness. The fever diminishes. The diet is to be of a light, nutritious character.

It was the most efficient and the simplest of any treatment which he had employed. The cardinal rule is to give the calomel until the condition of catharsis described has been produced. Under this method a greater number of cases will be saved than under any other proposed method.

DR. E. L. SHURLY remarked that he had a large experience with diphtheria and with the calomel treatment; and while he agreed that the internal administration of calomel is one of the most efficient remedies for croupous inflammation, he could not agree in regard to its efficiency in the treatment of diphtheria. The difference between croupous inflammation and diphtheria is considerable. Diphtheria is an infectious disease, while croupous inflammation is not necessarily an infectious process. He knew of no cases of diphtheria relieved by the calomel treatment.

DR. FRANK DONALDSON wished that Dr. Daly had given some statistics and reported some cases. He had seen the calomel treatment frequently tried, but without success. Trousseau treated cases of diphtheria in that way, but he finally abandoned it. If any mercurial was used, he thought that the bichloride would be the one to be employed. Under our present view of the nature of the disease, the best method of treatment would probably be the use of a spray of a solution of the bichloride of mercury (1 : 1000), sustaining measures, and keeping the patient in a room where there was free ventilation.

DR. BEVERLY ROBINSON, of New York, referred to the fact that there is scarcely a drug which has not at some time been recommended as a specific for diphtheria. Calomel has been thoroughly used, and finally given up because it did not accomplish what was proposed. He was convinced that epidemics of diphtheria as of other infectious diseases differ very much in virulency, and a remedy which is successful in one epidemic will entirely fail in another. Another point not sufficiently dwelt upon, as a rule, is that the characters of diseases differ much in different countries. He had never seen in New York such types of typhoid fever as he had seen abroad, notably in Paris. He had never seen such cases of malignant diphtheria in the hospitals of New York as in the children's hospitals of Paris.

He thought it safer to follow what has been considered by the accurate men of our profession, in large numbers, the most reliable treatment for a disease of this character. There is a general consensus of opinion

that the proper line of treatment is disinfection of the throat and air-passages, with the use of smaller or larger doses of tincture of iron, combined or not with chlorate of potassium.

DR. F. H. BOSWORTH, of New York, thought that mercury possesses a specific value in the treatment of diphtheria. Diphtheria is a blood poison, which often kills by its toxic effect, but in a large majority of cases it kills by leading to the development of croup. The affection of the larynx is not diphtheria, but a sequent disease. While mercury has no effect upon diphtheria, he believes that it has a specific effect in arresting the disease in the larynx, called croup. Mercury is the remedy to be administered in croup.

DR. J. N. MACKENZIE called attention to the fact that where, in diphtheria, we have great depression, alcohol should be our sheet-anchor.

DR. WILLIAM H. DALY, in closing the discussion, said that he was not surprised that this method of treatment had not been received with favor. He had not received it favorably when it was presented to him. He did not recommend calomel as a specific, but simply said that a greater proportion of successes had followed this plan of treatment than any other which he had tried.

DR. E. CARROLL MORGAN, of Washington, then read a paper on

THE QUESTION OF HEMORRHAGE AFTER UVULOTOMY, WITH THE DESCRIPTION OF AN INSTRUMENT FOR ITS ARREST.

The speaker first quoted from numerous authors, giving their views as to the danger of hemorrhage after this operation. He then described the following case: A man, aged twenty-eight years, whose uvula had been excised five days previously by another operator, came to him with the statement that the bleeding came on four hours after the operation, and had continued at intervals since then, a large quantity of blood having been lost. Various means had been tried to stop the bleeding, without success. The parts were covered with a coagulum induced by the application of the persulphate of iron. This was removed and the parts cleansed. The bleeding points were then seen. The uvula evidently had been greatly hypertrophied. The scissors had been used in the operation. There was no evidence of the hemorrhagic diathesis in this patient or in any member of the family. The parts being cleaned, the stump was seized with dressing forceps and the hemorrhage was controlled. The galvanocautery was applied and the bleeding stopped. In nine hours it recurred. The cautery was again used and the stump dusted with the persulphate of iron. The following morning a copious hemorrhage took place, not less than a drachm a minute, by actual measurement, being lost. A careful examination was then made to detect any other source of hemorrhage, but none could be found. Chromic acid was then used and the hemorrhage stopped. At three o'clock, the same day, copious bleeding recommenced. The galvanocautery was again employed. There was not room for the application of a ligature. Torsion was tried without success. A small clamp used in retaining shirt sleeves in position, the spring having been weakened and the teeth filed down, was then applied. A string was attached to this and brought out of the mouth. This checked the bleeding, and was allowed to remain for several hours. It was then removed and the bleeding did not recur. A search of the literature of the subject failed to show any instance in which such a procedure had been before adopted. In this case he thought that this device had saved the patient's life.

A careful study of the subject had enabled him to find seventeen other cases in which the hemorrhage after this operation had been profuse. Death has never resulted directly from the hemorrhage after uvulotomy.

A persistent, alarming hemorrhage is only encountered in the rarest instances. A moderate bleeding, stopping spontaneously or by the use of the mildest applications, occasionally happens. A loss of a few drops of blood at the time of operation, followed by a slight oozing, is a common occurrence. The most reliable surgical measures for controlling uvular hemorrhage, are ligature, compression by a clamp or forceps, and the use of the actual cautery. The most reliable styptics are in the order named: solid nitrate of silver, persulphate of iron, gallic acid, tannic acid, alum, the local use of ice, and vinegar. The most reliable systemic remedies are opium, acetate of lead, sulphuric acid, and ergot. In operating, the object should be to restore the uvula as nearly as possible to its normal size, and that the entire uvula should not be removed.

DR. F. H. HOOPER had never seen a bleeding of more than a few drops after this operation. He never cut through the muscle, and thinks that the removal of a small portion probably does as much good as the removal of a larger portion.

DR. FRANK DONALDSON held that the uvula very rarely requires to be amputated. The old idea that enlargement of the uvula produces irritative cough, is erroneous. The principal trouble produced by an enlarged uvula is interference with the proper adaptation of the soft palate during the second stage of deglutition.

DR. WILLIAM H. DALY, of Pittsburg, said that this operation, if done at all, should be so performed as to leave the parts in as nearly a normal position as possible. Uvulotomy is, however, rarely necessary.

AFTERNOON SESSION.

DR. THOMAS R. FRENCH, of Brooklyn, presented a paper on

THE LARYNGEAL IMAGE AS SEEN IN PHOTOGRAPHS TAKEN DURING THE PRODUCTION OF TONES IN THE SINGING VOICE.

The speaker had made numerous investigations on this subject, and his observations failed to confirm the statements of most authorities in regard to the position of the vocal cords during the production of different notes of the singing voice. Different indiv duals evidently use different mechanisms in the production of different tones. The general result of his investigations seemed to indicate that in the production of low notes, the vocal cords are separated to a greater extent posteriorly than anteriorly; in the production of the middle tones, the vocal cords are parallel; and in the production of high notes, the opening is widest in front.

The paper was illustrated by numerous photographs thrown upon the screen.

The following papers were then read by title:

Clinical Notes on Prolapse of the Laryngeal Ventricles, by DR. GEORGE W. MAJOR, of Montreal.

Three Cases of Thyrotomy: Recovery in Each Case with Good Voice, by DR. CLINTON WAGNER, of New York.

Alarming Hemorrhage after Tonsillar Excision, Arrested by Torsion of the Artery, by the same author.

DR. WILLIAM C. JARVIS, of New York, read a description of

A NOVEL PROCEDURE FOR THE REMOVAL OF SUBGLOTTIC LARYNGEAL GROWTHS.

J. C., aged thirty-five years, consulted him on March 22, 1882, for the relief of a difficulty of speech and of breathing. The trouble with the voice was noticed two years before, the difficulty in speech appeared one year before coming under observation. The laryngoscope showed a mass of papillomatous tissue occupying the cavity of the larynx. The vocal cords and ventricular bands were not involved. An attempt was made to seize the growth during phonation, with the Mackenzie forceps, but only a few pieces were removed. The écraseur was then tried, but it was found impossible to use this instrument on account of the wire being seized by the spasmodic contraction of the vocal bands and displaced. Chromic acid applications were then tried, but although the growth was touched, it was found impossible to limit the application to the diseased structures. The patient then disappeared, and did not return for a year. His condition was then much worse, and the difficulty of breathing greatly increased.

Dr. Jarvis then determined to try the following plan, and if this failed, to remove the growth through an opening in the windpipe. A forceps, similar to the Mackenzie instrument, but heavier, was secured. Through the tips of the blades holes were drilled, and through these was passed a piece of No. oo piano wire, which was conducted through a second opening at the angle of the blades, and out to the handles, where it terminated in a loop, into which the extremity of the index-finger could be placed. When the blades were separated, this wire formed a cross-bar uniting their extremities. When the blades were closed, the loop of wire could be drawn up by the finger. The operation was performed without an anæsthetic. The tongue was depressed, and with the aid of the mirror the forceps was introduced into the cavity of the larynx. The mirror was then removed and the forceps carried down to the glottis, through which the tip of the instrument was forced with considerable trouble, on account of the spasm. The instrument was carried into the trachea sufficiently far to be sure that it was below the attachment of the growth. The blades were then separated, and pressing against the anterior wall of the larynx, the instrument was gradually raised until the wire was caught. The blades were then closed and clamped, the loop of wire drawn in, and the forceps removed, bringing with it a growth. The laryngoscope showed a second growth, which was removed in the same way. Breathing was at once rendered easy, and the speech became natural. The patient said that the presence of the forceps, although uncomfortable, caused no pain. This is the only case on record of the removal of a subglottic tumor without anæsthesia, and without an opening into the larynx.

DR. T. AMORY DE BLOIS, of Boston, presented an account of

CASES OF LARYNGEAL ŒDEMA.

The author had been led to write the paper by reading of several fatal cases of this affection reported during the past few months. He gave brief details of fourteen cases of laryngeal œdema coming under his observation. All of these cases recovered, six under the use of astringents and eight after scarification.

DR. FRANK DONALDSON suggested that in these cases intubation of the larynx after the O'Dwyer method might be applicable.

DR. E. CARROL MORGAN, of Washington, stated that his experience with this condition had been limited. He had usually employed active purgation with vesication over the larynx and various local measures.

DR. W. C. JARVIS, of New York, referred to the constitutional treatment of œdema of larynx. Some time ago in a case of marked œdema, he used the fluid extract of jaborandi. In forty-eight hours the œdema was greatly diminished. Since then there have been several cases reported in which pilocarpin was used with advantage. He had never seen good results from scarification. In a case of attempted suicide followed by œdema of the larynx, a tube was introduced, and in this case although plenty of time could be taken, no benefit followed scarification. It seemed as though the fluid was contained in numerous small sacs. The opening of a few of these did no good.

A paper by DR. CHARLES H. KNIGHT, of New York, describing *A Case of Perichondritis of the Larynx*, was read by title.

DR. EDGAR HOLDEN, of Newark, described

A CASE OF GUMMATOUS DISEASE OF THE LARYNX, WITH SPONTANEOUS REOPENING OF THE LARYNX AFTER THYROID LARYNGOTOMY.

L. D., aged thirty-five, presented himself, May 20, 1885, with dyspnœa and aphonia. Examination showed a cicatrix on the epiglottis, and the left side of the larynx presented a rounded mass of dull appearance. He denied syphilitic infection. After a careful consideration of the case, the probabilities were thought to favor syphilis, and he was given iodide of potassium in large doses. There was improvement for a short time, but he soon became intolerant to the drug. Thyroid laryngotomy was considered necessary and was performed. The tumor was removed with the écraseur; no ulceration was visible. Tracheotomy was then performed and the upper incision closed with sutures. The patient did well and the tube was removed on the tenth day. The external wound healed. He insisted on returning to his home, where the surroundings were very unfavorable, and his health deteriorated. On the fortieth day after the healing of the wound, the laryngeal incision was torn open by a severe cough. He refused to return to the hospital. Examination of the larynx showed no disease. Owing to the surroundings it was impossible to get the wound to heal. His health continued to run down, and six months and fourteen days after the operation, the patient died of exhaustion, having lived five months with an opening in the larynx.

DR. U. G. HITCHCOCK, of New York, read a paper on

GUMMATOUS INFILTRATION OF THE BASE OF THE TONGUE,

giving the details of four cases coming under his observation. In none of these cases was the fibrous septum

the exclusive seat of the disease, and in three cases the deposit did not approach this structure. In these cases the degree of dysphagia was in direct proportion to the proximity of the deposit to the base of the tongue.

(To be concluded.)

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, May 6, 1886.

THE PRESIDENT, B. F. BAER, M.D., IN THE CHAIR.

SÄNGER'S CÆSAREAN OPERATION.

DR. ROBERT P. HARRIS desired to give publicity to the following statement, received a few days ago in a letter from Dr. Sänger, of Leipzig, by which it will be seen that his method now stands unrivalled in the world in its ability to save human life. Locally considered, the Porro operation as performed in the Santa Caterina Hospital of Milan, Italy, has, until recently, far exceeded in its proportionate success all other Cæsarean methods, in any hospital or country, but this, the best of all Porro successes, has now to be rated second, as compared with its younger German rival. Laparoelytrotomy, a year ago, stood upon the same level with the Sänger operation in its rate of success; but now the latter far outstrips it in the number of times it has been performed, and in its proportion of cures. According to Dr. Sänger's letter, his operation, with its modifications and simplifications, has been performed 25 times, saving 18 women, or 72 per cent., and resulting in 22 children being delivered alive, or 88 per cent. In these are included three fatal American cases, which if not in an absolutely hopeless state before the operation, gave a very minimum hope of success. The 22 European operations saved 18 women, or $81\frac{9}{11}$ per cent. In the Maternity Hospital of Leipzig, Dr. Sänger has operated four times, Dr. Obermann once, and Dr. Donal once, saving all of the women and children; in but one woman was there any special trouble after the operation. Dr. Leopold, of the Dresden Maternity Hospital, has operated nine times, and Dr. Korn once; the former lost one women: all of the children were saved. Thus we have 15 women and 16 children saved under 16 operations, a mortality for the former of only $6\frac{1}{4}$ per cent. Of the four deaths in Europe, two resulted from septic poisoning which existed at the time of the operation and in the other two subjects it followed it.

Dr. Sänger has such confidence in his method from the success that has attended it in Germany that he believes the time has come when it should be preferred to craniotomy, because of its moderate fatality, and its saving the child. We should be glad if all of the Cæsarean operations of the United States should be performed after Sänger's method as simplified by Garrigues and Leopold; but we must not accept very happy results here until our accouchèurs become alive to the fact, that delay in operating will make any method fatal in a large proportion of cases. In no country are the capabilities of the old Cæsarean operation greater than in the United States, and in few has this form of delivery been of late more uniformly fatal. To find 18 recoveries under it we must search backward to January, 1863, and through a record of time covering more than twenty-three years; in which period 73 operations have been performed, proving fatal in about 75 per cent. This occurred notwithstanding an established fact that a set of early operations will save 75 per cent. of the women and still higher of the children in the United States.

TAIT ON FARADIZATION.

DR. R. P. HARRIS also presented the following letter from Mr. Lawson Tait, dated April 16, 1886:

"I have very strong objections to the proposal to treat cases of extrauterine pregnancy by faradization. In the first place, the diagnosis of these cases must always be haphazard—that is to say, a correct diagnosis will not be made probably more than once in three times; the result will be that all such cases will be dealt with mischief only, and I venture to predict that this treatment will be dropped as all such treatments are, without explanation of the case in a very short time.

"My greatest objection is, that supposing the fœtus has passed through the stage of fœtal rupture and remained alive, what right have you to murder that child? If it goes on to the full time it may be delivered alive, and the woman will have a chance of recovery from the operation, far greater than with the faradization treatment of destroying the child. The cases, according to experience, which recover from the operation, are about six out of seven.

"Every one, who has had much experience with pelvic tumors, must have seen a certain number of cases where the fœtus has died between the fourth and sixth month, and where after a prolonged course of suppuration it comes out through the rectum, bladder, etc.; these are, of course, the cases where the tubal rupture has taken place into the broad ligament on the left side. I have seen one right-sided case going into the bladder; it, of course, killed the patient.

"In the whole course of my life I have only known of one case where the woman has carried an extrauterine pregnancy for a number of years after the death of the fœtus. We know with perfect certainty all about this case, and for about eighteen years she has carried on the left side a condensed ovum of extrauterine pregnancy. I doubt very much if there could be found in the whole world three other such cases; whereas, the number of cases who die or who have prolonged illnesses, after the suppuration and discharge of the fœtus, is even in my own experience very great."

In closing his letter Mr. Tait writes:

"I wish you would make this opinion of mine known on your side."

In reply, I will state: 1. We do not, in this country, practise electrolysis in cases of extrauterine pregnancy. No puncturing needles are used, and the electro-magnetic current will not endanger the life of the patient any more, if the growth to be acted upon is a tumor, than if it be, as presumed, an ectopic fœtal cyst. The experience of seventeen years in the United States, in which no fatal result is believed to have taken place, has only tended to establish this fœticidal method as a valuable method of saving women when in great danger of death from rupture of the fœtal cyst and internal hemorrhage.

2. We do not propose to act upon the fœtus after it has escaped into the abdominal cavity, unless the fœtus should be very small, and be easily accessible to the pole of the battery placed in the vagina. We cannot

see that it is any more a murder to destroy a two or three months fœtus after it has escaped from a Fallopian tube by rupture than while it is still in it. The chief objection lies in the fact that such an ectopic fœtus will be much more likely to give trouble after its destruction than one that is securely enclosed in a sac from which the amniotic fluid shall have been absorbed. It is true that an abdominal fœtus may be delivered alive at term, if permitted to live; but it is not correct to estimate the risk of such an operation as lower than faradization properly performed, for it is far higher. Primary laparotomy, so far as we know of the operations, has been fatal in fifteen out of nineteen cases.

It is not proposed in this country to operate by faradization upon fœtuses of from four to six months. Dr. T. G. Thomas has, it is true, proposed to make the limit four and a half months, but the general impression is, that fœticide is much safer immediately, and remotely, if done in the second and third months, when fœtal ossification is very incomplete. The entrance of fœtal débris into the bladder is not necessarily fatal, as in the case related by Mr. Tait, for Parry refers to nine cases, four of which recovered.

Mr. Tait appears not to be aware of the fact that cases of prolonged ectopic gestation have been comparatively numerous, as witness the following partial record:

1. Nebel reports the case of a woman of ninety-one, who died in 1767, and in whose body a fœtus was found, that she had carried fifty-five years. (Campbell on *Extrauterine Pregnancy*, 1840, page 45.)

2. Brandt records one of a woman of eighty, who died in 1858, after carrying a fœtus for fifty-six years, and bore two children while it was still in her abdomen. (Ranking's *Abstract*, 1863, vol. i. page 216.)

3. Parkhurst reports one of a woman of seventy-seven, who carried a fœtus fifty-two years. (*British and Foreign Med. Chir. Rev.*, 1856, vol. i. page 271.)

4. Chiari gives a case of a patient who died of pneumonia when eighty-two, who carried an eight months fœtus for fifty years. (*Lancet*, London, 1876, vol. ii. page 141.)

5. Conant's case was a woman of sixty-three, who died in June, 1863, after carrying a fœtus thirty-five years. (*New York Med. Journ.*, May, 1865.)

6. Major found in a woman of seventy-eight a calcareous fœtus computed at three months. (Cruveilhier, *Essai sur l'Anatomie Pathologique*, Paris, 1816, tome ii. page 130.)

7. Mangin Dernier found two fœtuses in the body of a woman of seventy-four, which she had carried thirty-three years. (*Journ. de Méd.*, 1786; *Gaz. Méd.*, July 29, 1837.)

8. Morand, also, found a three months' fœtus in a woman of seventy-eight; she had carried it for thirty years. (*Mém. de l'Acad. Roy. des Sciences*, 1748.)

9. Kristian Grön found a three months' fœtus in a woman of forty-nine, which had been carried eighteen years. (*Norsk Magazin for Lægevidensk*, Band xvii. Heft 2.)

10. Johannis Ambosi (1582) reports a case of a woman of Lens, who carried a fœtus twenty-eight years. (See Astrus, *Traité des Mal. des Femmes*, Paris, 1765, tome iv. p. 78.)

11. Campbell reports the case of a woman of seventy-five, in whom was found a fœtus that had been carried thirty years; a fœtus of about two months was also found. (Campbell on *Extrauterine Gestation*. Edin., 1840, p. 55.)

12. Pepper relates the case of a patient of fifty-three, married twenty-seven years, in whose body Dr. Loder found two fœtuses, one carried twenty-three years. (*Trans. Pathol. Soc. Phila.*, 1876, p. 102.)

13. Francis Bayle removed an eight pound fœtus after the death of a woman of Toulouse, that she had carried twenty-six years. (*Philos. Trans.*, abridged, London, 1794, vol. iii. p. 222.)

14. Cruveilhier, in his *Anatomie Pathologique*, gives a plate representation of a fœtus which had been carried many years.

15. Œlinger reports the case of a woman who carried a six and a half months' fœtus about fifteen years. (*Prog. Méd.*, Paris, 1884, vol. xii. p. 196.)

16. Johnson's case, aged sixty-eight, carried a fœtus fourteen years, after which she discharged fœtal remains at intervals during thirty years. (*Med. Times*, London, 1872, vol. i. p. 655.)

17. Leinzell, in 1720, removed from the body of a woman of ninety-four a fœtus that she had carried for forty-six years.

18. Watkins examined a woman of seventy-four, who died of kidney disease January 13, 1866, and removed a fœtus which she had carried for forty-three years. (*Brit. Med. Journ.*, March 3, 1866.)

19. Van Sweiten, also, records the case of a woman of Lyons, who died at sixty-eight, and had carried a fœtus for twenty-seven years. (*Opus cit.*)

20. Fabri, of Ravenna, found in a woman of fifty-five a fœtus she had carried for some years. The pregnancy was her fifth, and she bore two children at later periods. (*Brit. Med. Journ.*, March 7, 1863.)

Many more cases of the same character might be added to this list, some of which would go to show that an extrauterine fœtus may prove fatal by purulent disintegration and pointing after twenty years or more. Even an ectopic fœtus of three months may cause perforation of the rectum, and possibly a fatal issue, although this is a rare result. It will be noticed that in three of the twenty cases the fœtus was computed at three months, and in another a second fœtus was of two months.

NEW YORK COUNTY MEDICAL ASSOCIATION.

Stated Meeting, May 17, 1886.

THE PRESIDENT, CHARLES A. LEALE, M.D., IN THE CHAIR.

DR. LEWIS HALL SAYRE read a paper entitled

A CONTRIBUTION TO THE ETIOLOGY OF POTT'S DISEASE.

The symptoms accompanying spondylitis in its earliest stages, he said, are vague, and not of a character to call attention directly to the spine; and, unless the examinations of the patient are conducted with great care and minuteness, the disease is often not detected before the lapse of many months, and not until irremediable deformity has occurred. Dr Sayre said that he sided with those who look upon traumatism as the exciting and necessary cause, which, in a constitution already vitiated, must be much less severe to produce spinal or other lesions than in a person of vigorous and robust health.

The disease in its inception attacks the bony part of the spine in its anterior portion, and is at first an inflammatory process. This is followed by absorption or necrosis, and the formation of pus and abscesses in many cases, and finally ankylosis of two or more vertebræ results, after a period of treatment of longer or shorter duration. An early diagnosis is of the greatest possible importance. In many of the text-books it is stated that pressure upon the spinous processes causes pain in spondylitis, but this is very often not the case, as this pressure separates the bodies of the vertebræ, and, by removing the inflamed surfaces from contact with each other, gives relief, instead of pain.

There is another point of great importance in the examination, and it is one which, with two exceptions, is not mentioned in any of the standard works on this subject. There are certain cases in which the disease commences on the *sides* of the bodies of the vertebræ, at the point of their articulation with the ribs, and not in the anterior part of the bodies, as is commonly the case. They are usually caused by some violence applied to the rib, which originates an inflammation at its articulating facet on the spinal column.

These cases are more than ordinarily obscure in the commencement, and are difficult of diagnosis, as pressure on the head seldom causes pain, and the child can run, and sometimes jump, without pain, and can often bend the body without inconvenience, while it may have very little muscular rigidity.

In order to detect this condition, it is necessary to place the child across the lap, making strong pressure upon each rib, so as to bring it into firm contact with its articulating vertebra; and when the rib is reached which articulates with the inflamed or diseased vertebra, either pain will be produced, or a muscular spasm, very similar to that caused by "point" pressure on a "contractured" tissue, will take place. These manifestations of trouble may also be produced by pressure made on either side of the spine over these articulations. In conclusion, Dr. Sayre related a case of great interest illustrating this form of spondylitis.

DR. LEWIS A. SAYRE remarked that the case described by his son was one of the most interesting that he had ever seen. It had been pronounced by a well-known neurologist to be one of organic disease of the cord, and there was absolutely no deformity that could be detected by the most careful examination. He himself thought there was no spondylitis until he made a lateral examination, and found that pressure over the vertebral articulation with the eleventh rib caused acute pain. When this point of tenderness was discovered, it became evident that there was disease at the side of the vertebra, and a plaster-of-Paris jacket was accordingly applied. Owing to the fact that the patient indulged in violent exercise before the latter was perfectly "set," it soon broke down, and was removed at the end of two weeks, when a distinct knuckle was formed. This proved that the diagnosis was correct; but if the first jacket had been perfect, it was doubtful whether any deformity whatever would have occurred, and the diagnosis of Pott's disease would have been open to question. The deformity, however, was now so slight that it would be likely to escape observation entirely under ordinary circumstances. This subject he considered one of vast importance.

DR. J. W. S. GOULEY exhibited a peach-pit which he said was of interest as constituting a

CONTRIBUTION TO THE SUBJECT OF FOREIGN BODIES IN THE ALIMENTARY CANAL.

A gentleman, seventy-one years of age, had removed it from his rectum three months after it had been swallowed. It was an inch and a quarter in length, seven-eighths of an inch in breadth, and eleven-sixteenths of an inch in thickness. When it was extracted from the rectum, the patient remembered that three months before he had eaten some pickled peaches, since which time he had eaten no peaches at all. The case was interesting from the fact that the peach-pit caused no pain or distress whatever in passing through the intestinal canal.

Dr. Gouley then went on to say that much larger foreign bodies than this peach-pit had been known to pass through the pyloric orifice without pain. In Eve's *Surgical Cases*, page 258, an account is given of a large egg-cup which passed successfully through it and the ileocæcal valve, but which finally lodged in the ileum, about ten inches beyond the latter point. The patient died three months afterward, death being due, in part, to a strangulated hernia. Another remarkable case recorded in Dr. Eve's work is one reported by Otto, of Copenhagen, in which the patient swallowed a silver spoon, which was discharged twelve months afterward through an ulceration in the præcardial region, recovery following.

Dr. Gouley then related a case of his own, which occurred twenty-five years ago at Bellevue Hospital. In a patient suffering from delirium tremens, who died shortly after admission, there were certain heart symptoms which could not be satisfactorily explained, though there were some signs of pericarditis. At the autopsy the pericardium was found filled with flaky lymph, and when the heart was lifted up a metallic substance was felt. On opening the œsophagus, this was found to be a plate, with two teeth attached.

A case reflecting the highest honor on the surgeon who had charge of it was one operated on by Dr. Samuel White, of Hudson, New York, in 1807. A patient swallowed a teaspoon, and it was successfully extracted one month afterward by Dr. White, who performed laparotomy, made an incision in the ileum, and, after removing the spoon, sewed up the intestine and the external abdominal wound. A good recovery was made. In conclusion, Dr. Gouley related some curious cases reported by Velpeau and others, in two of which iron forks were swallowed.

DR. JOHN H. HINTON mentioned that a negro man in his employ once swallowed a peach-pit. He took a dose of castor-oil, and the next day passed the pit by the anus without any difficulty. The case was remarkable on account of the extremely rapid progress of the foreign body through the alimentary canal.

DR. LEWIS A. SAYRE related two cases of his own in which two elderly gentlemen swallowed sharp pieces of oyster-shell about the size of a shilling, the foreign bodies giving no trouble until they got into the rectum, when they produced a considerable amount of irritation. Dr. Sayre also mentioned some cases illustrating the use of the bristle or umbrella probang in the extraction of foreign bodies from the œsophagus. For a long

time he supposed that this device was original with him, but he afterward learned that it had been previously employed by an officer in the British East India service, who had not, however, made it public. The first case in which he used a bristle probang was that of a young man who had swallowed a tooth attached to two hooks, which caught in either side of the œsophagus.

DR. P. BRYNBERG PORTER mentioned a case recently reported by Dr. C. B. Hutchings, of California, in which an open penknife passed successfully through the alimentary canal. The patient, a young man of twenty, accidentally swallowed the knife, handle first. He was ordered a dose of oil by the physician who first saw him, but this was fortunately not taken. Dr. Hutchings instructed him to eat freely, particularly of mush and buckwheat cakes, and to lie constantly on the right side, with the hips elevated, so as to facilitate the passage of the knife into the duodenum. The patient claimed that he felt the knife at various points along the intestinal track, and that it caused considerable pain when passing through the ileocæcal valve. It was finally discharged, during a very large movement of the bowels, on the morning of the sixth day, with the point first. Dr. Hutchings believes that in all cases where a rough or sharp instrument has been swallowed a bulky, non-concentrated food should be given for the purpose of filling and distending the bowels, thus preventing the object from catching in any of the numerous folds of the intestines. In this instance the knife, with the blade, measured three and a half inches; while the point was sharp, and the rivets in the handle projected a sixteenth of an inch, owing to the worn condition of the latter.

Dr. Porter also referred to a similar and later case, in which the same plan of treatment had resulted in the successful passage of an open toy-knife, considerably smaller than the one just mentioned; and, in addition, to a case reported by Dr. Gerster, of New York, in which a metallic button, three-quarters of a inch in diameter, was swallowed by an infant six months of age without the mother's knowledge. On account of increasing dyspnœa and failing health, tracheotomy was performed six months later, under the supposition that the trouble was due to a laryngeal tumor. As at this time the button was firmly embedded in the tissues between the œsophagus and trachea, it was not discovered at the examination made immediately after the throat was opened, and three days afterward the infant died of pneumonia.

THE PRESIDENT thought it a point of importance to determine, as far as possible, how large a foreign body could pass the ileocæcal valve. Some years ago he was called to see a child nine months of age who had swallowed a two-cent piece, which, as it first lodged in the œsophagus, the mother had pushed with her finger down into the stomach. While keeping the infant under observation he made an investigation on the cadaver, and found that by keeping the coin flat a two-cent piece could be made to pass the ileocæcal valve of a child of nine months. About two weeks after swallowing the foreign body the infant had a convulsion; after which it had no further trouble until some time later, when it was noticed that it seemed to suffer considerable pain whenever it had a passage. This was due to the irritation caused by the coin in the rectum, and shortly afterward it was successfully passed. When a patient swallows a foreign substance which is so large that it is impossible for it to pass the ileocæcal valve, the question arises whether we shall allow it to ulcerate out or perform laparotomy. The successful case of Dr. Samuel White, related by Dr. Gouley, would seem to indicate that the latter is the preferable course.

THE PHILADELPHIA NEUROLOGICAL SOCIETY.

Stated Meeting, March 22, 1886.

THE PRESIDENT, S. WEIR MITCHELL, M.D., IN THE CHAIR.

DR. H. C. WOOD read a paper on

LANDRY'S PARALYSIS.

In the course of the paper the symptomatology and diagnosis of neuritis and poliomyelitis were to some extent considered.

DR. G. BETTON MASSEY said that, in regard to pain on pressure in poliomyelitis, it is of interest to recall the fact that in this affection the skin is unusually sensitive. In children, for a year after the attack, there is hyperæsthesia to tactile impressions and especially to the battery. Hyperalgesia would probably better describe the condition. These cases have presented marked reactions of degeneration, and it has seemed to him that the greater the reactions of degeneration the greater the sensitiveness to the galvanic current in children. An autopsy is by no means necessary to make out the diagnosis, for the clinical histories of these cases are almost identical. It should be as easy to diagnose acute poliomyelitis in a child as to diagnose smallpox. He thought it might be possible, in the cases referred to, to remove a portion of the nerve for examination during life.

DR. WHARTON SINKLER, in reply to Dr. Wood's statement that the diagnosis cannot be regarded as proved unless there has been an autopsy, said that we cannot make autopsies in all cases, and we must therefore infer that our diagnosis is correct when the symptoms are identical with those of other cases in which autopsies have been made. Competent observers in this country and abroad have made post-mortem examinations in cases of infantile paralysis, and have found distinct lesions in the anterior horns of the cord. Hence we may assume that there is such a disease as poliomyelitis anterior. He had seen children, as described by Dr. Massey, in whom there was hyperæsthesia to electricity, especially to the galvanic current.

At least twelve or fifteen years ago Barnwell advanced the view that infantile paralysis was a disease of the peripheral nerves; the later observers, supported by autopsies, seemed to have disproved his theory. Kennedy has described a variety of infantile paralysis under the name of temporary paralysis. We believe that this disease is distinct from "myelitis of the anterior horns," and it may be the result of a diffused neuritis, as suggested by Dr. Wood. He had no doubt that some of the cases of the so-called spinal paralysis of adults, those of short duration, are of a similar nature.

DR. FRANCIS DERCUM had lately seen a case in private practice which was of interest in this connection. A man who had been exposed to damp and cold had a chill followed by fever. He was called to see him, and found him sufferring with intense pain affecting both brachial plexuses, followed by rapidly developing pare-

sis of both arms. This was evidently a case of peripheral neuritis. The electrical reactions were examined by Dr. Lloyd, who can better give the results of this examination.

In such a case, if the patient does not die early, we may expect the cord to become involved, and then, in the absence of a knowledge of the previous history, it would be impossible for any one to say which was the primary lesion.

He thought it possible that we may have a disease in which both the centres and the nerve trunks are affected simultaneously. Diphtheritic paralysis is probably a case in point.

DR. JAMES HENDRIE LLOYD thought that the case referred to by Dr. Dercum was of interest, especially with reference to one or two points which had been brought forward in regard to the reactions of degenerations. This man was completely paralyzed in both deltoid muscles, and in the right infraspinatus and the right teres major and minor muscles. The left infraspinatus and teres muscles were perfectly normal. If this was a case of peripheral neuritis, we have facts contradicting some observations made to-night. In this case, typical reactions of degeneration were present. The changes were both serial and modal, and the faradic irritability was almost extinct. There were no fibrillary contractions in this case. In regard to this subject of reactions in wasting muscles, it seems to him that where there have been marked fibrillary contractions, the reactions of degeneration are not as distinct as we should expect to find them, and this is probably due to the fact that such cases are caused by a slowly advancing anterior poliomyelitis, in which the trophic centres are very gradually destroyed; whereas, in peripheral neuritis the lesion would be more quickly established, and the characteristic electrical changes would be earlier observed.

DR. CHARLES K. MILLS thought that while multiple neuritis may occur more frequently than we have been led to think, still, we have as yet few positive diagnostic points. Pain on pressure over a nerve trunk is perhaps one of the best signs of neuritis of all forms, but this is also sometimes found in subacute myelitis of the anterior cells, and in neuralgias there is hyperæsthesia at limited points. In genuine posterior sclerosis at times limited areas of pain on pressure are present. In regard to Landry's paralysis, it seems that subacute myelitis of the anterior horns would simulate it more closely than anything else.

Some years ago, Leyden advanced the view that locomotor ataxia was due to neuritis, and since then this view has been advocated by different authorities. We find tenderness over the nerves in various spinal and cerebral affections. In some cases of brain tumor, hyperæsthesia is a marked feature. The same is sometimes seen in spinal tumors. It might be said that in these cases there was also neuritis, but in some cases of brain tumor this is impossible. The tumor may be so high as to be remote from the nuclei of origin of the nerves. Leaving post-mortems aside, the uniform bilateral character of many of the cases classed as subacute anterior poliomyelitis is, so far as it goes, in favor of a central origin of the disease.

The suggestion of Dr. Dercum that the disease may simultaneously affect the nerve trunks, and the centres seems more philosophical than to attribute all these cases to multiple neuritis. In arsenical paralysis, he believed that the nervous protoplasm was everywhere affected.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, June 8, 1886.

THE PRESIDENT, A. JACOBI, M.D., IN THE CHAIR.

DR. CHARLES CARROLL LEE read a paper on

THE FUTURE INFLUENCE OF SURGERY AND SURGICAL RESOURCES UPON OBSTETRICS.

He gave a review of what had been accomplished in obstetrics from a surgical aspect, and spoke first of Cæsarean section. He spoke of the four modifications of it suggested respectively by Franck, of Cologne, Kehrer, of Heidelberg, Sänger, of Leipsic, and Cohnstein, of Heidelberg. Kehrer's plan is described as consisting of a transverse incision through the anterior convex surface of the uterus at the level of the os internum, of deep sutures for the muscular walls, and superficial sutures to secure immediate contact of the peritoneal borders, and of strict antiseptic precautions during the operation, combined with subsequent drainage of the abdominal cavity. In order to cover the line of incision with peritoneum, he directs that the latter be separated from the underlying muscle for nearly a half inch from the borders of the wound.

Sänger suggested that, after the removal of the child through the ordinary incision, and a temporary elastic ligature had been placed around the cervix, a narrow strip of muscular tissue should be removed by a section running at first parallel to the borders of the wound by diverging outward as the section approached the peritoneal surface. The peritoneum is next to be detached and allowed to cover the expanded portion of the Y-shaped section. He then advised the introduction of deep uterine sutures running obliquely through the uterine tissues to the inner border of the wound, avoiding, however, the decidua, with a second set of superficial sutures designed to secure the contact of the peritoneal borders. This principle of "subperitoneal muscular resection," as it is called, has been practised by Leopold, who found it necessary, however, to make certain modifications in its application.

The method proposed by Cohnstein is to turn the uterus completely out of the abdomen, and open it from behind. This, so far as Dr. Lee is aware, has never been attempted in actual practice.

Having quoted from Dr. Robert Harris, of Philadelphia, the highest American authority on Cæsarean section, to show that the greatest danger about this procedure arose from the delay commonly made in resorting to it, Dr. Lee spoke of the substitutes which had been devised to take the place of Cæsarean section. The first of these was Porro's operation, ovarohysterectomy, involving the removal, after the performance of the Cæsarean section, of both uterus and ovaries. This was afterward modified by Müller, who recommended that the uterus, after the enlarging of the abdominal wound, should be tilted laterally, and withdrawn outside the abdomen previous to section.

The other substitute for the Cæsarean section was laparoelytrotomy, as suggested and successfully prac-

tised by Dr. T. Gaillard Thomas, in which the dangers of opening into the peritoneum and wounding the uterus are avoided by incising the walls of the abdomen in the line of Poupart's ligament, lifting the peritoneum, dissecting down to the vagina, dividing the vagina transversely, and then, having reached the cervix, extracting the child through the passage thus artificially created. Although the number of cases in which laparoelytrotomy has been performed is as yet comparatively small, the proportion of successes thus far is much greater than has been met with either in the Cæsarean section, or the Porro or Porro-Müller operation. During his remarks on this subject Dr. Lee quoted a number of statistics, the most of them from Dr. Harris's recent article in the *International Encyclopædia of Surgery*.

He next took up the topic of extrauterine pregnancy; and showed what excellent results had been accomplished by the use of electricity in the earlier months of ectopic gestation, as first successfully employed by Dr. J. G. Allen, of Philadelphia. Having spoken also of the results obtained by elytrotomy and laparotomy in the later months, he went on to speak of laceration of the cervix as a result of parturition. Personally, he said, he had never been successful in securing union when he attempted to repair this lesion immediately after labor, and he knew of but one surgeon who claimed to get good results in this procedure. In a large number of cases which were left to nature no subsequent operation was required.

With regard to lacerations of the perineum, however, the case is very different. There the advantages of immediate closure are so immense that he regarded it as a matter of great importance, and he said that he would now as soon think of leaving a parturient woman with the child undelivered in the uterus as to neglect sewing up a torn perineum. The neglect to do this on the part of many practitioners is the source of much suffering and misery, which might have been avoided with very little trouble.

Notwithstanding the fact that operative surgery has already accomplished so much in the field of obstetrics, he feels confident that, judging from the triumphs of the last few years, the results noted will be still more satisfactory in the future.

As to lacerations of the cervix resulting from parturition, he had seen many serious cases, the rent perhaps extending to the vaginal roof, recover perfectly under ordinary treatment, without resorting to operation; and his views on this point were quite in agreement with those of Dr. Lee.

In the English hospitals, more than in this country, obstetricians have to fight their way surgically against the mere physician, and also to contend against the active opposition of those recognized as general surgeons. But the same is true to a considerable extent here also, and he was happy to say that the obstetrician, notwithstanding the indifference manifested on the one hand, and the active opposition on the other, had won for himself an enviable position.

He need only point to the matter of abdominal section. Surgeons complain that we open the abdominal cavity too often. It may be; but that is simply a side issue. Before the obstetricians of to-day lies the grandest problem that can possibly engage their attention, viz., that in connection with the Cæsarean section. Dr. Polk said he did not doubt that within the next two years it would be possible to eliminate entirely from practice the most repulsive of all obstetrical procedures, the destruction of the life of the fœtus by craniotomy. When this has been accomplished there will have been achieved the greatest work ever done in surgery, and the honor of it will belong to the obstetrician.

DR. FORDYCE BARKER said that he was not inclined to accept any new proposition until time and experience had determined its real merits; and for a long time he had held his opinion in regard to the immediate operation for lacerated perineum in abeyance. Some years ago, at a meeting of the American Gynecological Society, a distinguished obstetrical authority made the statement that any physician who did not close by primary operation a rupture of the perineum occurring in his practice was neglectful of his duty. He did not express any positive opinion in regard to the matter at the time; but he hesitated to accept this dictum as correct, and at the present time he was still less inclined to acquiesce in its sentiment. In no case in his private practice had a secondary operation for lacerated perineum ever been performed; and this was simply for the reason that it had never been required. He would not say that he had never had any perineal lacerations, but only that none of them were of such a character as to demand a secondary operation. The uterus retained its position perfectly in every instance, and no other bad results occurred from the rupture. He would say, still further, that up to the past winter, in no private patient of his had a primary operation for lacerated perineum ever been performed.

During the season just closed, from November 1st to May 1st, there has been a peculiar epidemic in this city. In consultation, he had seen forty such cases; but in his own practice only one had occurred. There were no symptoms whatever till about the sixth day, when an explosion occurred in the system, the temperature rapidly running to 104°, 105°, or 106°. Only one, however, of the patients that he had seen died. In the case of the others the symptoms all disappeared, as a rule, within three days. Out of the entire party, he was called to see only twenty more than once. It was a significant fact, he thought, that in five out of the forty cases the primary operation for lacerated perineum had been performed, and in only one of these did union take place. This was the case in which the patient died. In the one that occurred in his own practice, there was some contraction of the pelvis, and he delivered the child from the superior strait with the Simpson modification of Tarnier's forceps. Here there was an extensive laceration of the perineum, and the primary operation was performed; but union did not take place.

In two cases that he knew of, in which the primary operation was performed, there was profuse uterine hemorrhage as a result of shock. After referring to other cases, Dr. Barker said that his mature opinion is that while there are, no doubt, cases in which the condition of the patient is so favorable that primary union is likely to result from an immediate operation, as a rule it is safer to avoid the operation, and trust to absolute cleanliness, and watchful care of the patient. He could not, therefore, accept the prevalent teaching of the day, and he was glad to hear Dr. Polk express

himself as he did in regard to primary operations. If there is any tendency to septic trouble, he thinks the immediate sewing up of the perineum would be likely to be attended with unpleasant consequences on account of the interference with free drainage which was thus caused.

DR. MALCOLM MCLEAN said that he felt compelled to advocate the desirability of the primary operation in lacerations of the perineum. He had found that in those cases in which he sewed up the laceration with soft silk sutures the result was more satisfactory than in those which are left to nature. He does not believe, however, in using silver wire sutures, as they are apt to displace the parts; but silk sutures, deeply and carefully placed, are generally efficient and satisfactory, and they have the advantage, at all events, of never doing any mischief, even if they do not accomplish the desired result. Moreover, they serve a good purpose by their moral effect in backing up the accoucheur in case union does not take place, for in these days when so much is expected of one in the way of operative surgery, a reflection is apt to be cast upon the medical attendant if he neglects to take proper care of a lacerated perineum. In the main, however, he had found this simple operation of decided practical advantage.

DR. H. J. BOLDT spoke against the primary operation in lacerations of the cervix. If repair does not take place by nature, he said, it is easy to perform the secondary operation at any subsequent time that is convenient. He therefore protested against the immediate operation, unless there is a profuse hemorrhage due to the laceration of the cervix.

DR. POLK said that he wished to correct a misapprehension on the part of Dr. Barker. His remarks were made in reference to lacerations of the cervix, and not of the perineum.

In answer to an inquiry by Dr. Barker, as to what his present views were in regard to the primary operation for perineal lacerations, Dr. Polk said that he felt constrained to say that he entirely acquiesced in the opinion expressed by Dr. McLean. He would admit, however, that there are exceptions, in which the conditions, so well described by Dr. Barker, contraindicate the primary operation. There are many cases in which the accoucheur has had a bad operation, or labor has been unduly prolonged, where the existing condition of shock would be likely to be followed by hemorrhage; and here we are perfectly justified in declining to operate. We must always be sure that the patient has not already been subjected to the risk of shock before we make an attempt to repair the laceration.

Like Dr. McLean, he greatly preferred the silk suture, and it is now some time since he had abandoned the use of wire in either primary or secondary operations of this kind. Silk properly prepared, and soaked in a bichloride solution before using, is quite as efficient as silver wire. It was natural that the latter should have been preferred to all others before the days of antiseptic surgery; but now that we can summon antiseptics to our aid, we are able to dispense with the metallic suture, which was always a cause of great pain and annoyance to the patient. The withdrawing of the sutures, also, is always a source of more or less suffering.

DR. LEE, in closing the discussion, said that he differed from Dr. Barker in regard to the advisability of the closure of perineal lacerations. It might be that he had not been able to place his patients under the same favorable conditions for spontaneous recovery which the latter had; but, at all events, he had not been fortunate enough to obtain the same satisfactory results when such cases are left to nature. He then referred to the practice observed in the lying-in department of the New York Foundling Asylum, in which the patients were nearly all primiparæ. In this institution it is the uniform rule in every case, whether the patient is attended by the house-surgeon or one of the visiting physicians, that immediately after labor the condition of the perineum should be carefully ascertained, and if it was found to be decidedly torn, to close the laceration at once. Of course, he did not wish to be understood as advocating an operation where there was merely an abrasion, or a tearing of the fourchette, which is almost universally the case with primiparæ. He does not believe, either, in operating when there is profound shock present from loss of blood or other cause; but he had very rarely met with such a condition in his practice.

The plan which he adopts is simply to continue the chloroform under the moderate use of which the child has been born, and to make an examination of the perineum; when, if a decided rupture is found, the use of the chloroform is further continued, and the laceration repaired by means of silk sutures. The material which he uses is Skene's antiseptic silk, which he has found very efficient and satisfactory. Like Dr. Polk, he had not for some time employed silver wire sutures in either primary or secondary operations. If the posterior wall of the vagina is involved in the laceration, he usually puts in two or three catgut sutures at this point. The whole operation is a very simple one, and he had never seen shock caused by it. He always performs it under chloroform. In his practice at the Woman's Hospital, he has seen so many bad results, which might have been avoided, produced by neglect to perform the primary operation after rupture of the perineum, that he is most firmly convinced as to the great utility of the procedure.

NEW YORK SURGICAL SOCIETY.

Stated Meeting, May 24, 1886.

L. A. STIMSON, M.D., IN THE CHAIR.

RESECTION OF THE ELBOW-JOINT FOR ANKYLOSIS.

DR. SANDS presented a patient, fifteen years of age, who was seen by him last autumn, in consultation, in Dartmouth, N. S. The ankylosis at that time maintained the arm and forearm in almost a right line. The ankylosis, he thought, was almost complete, and had existed since the boy was about two years of age. The usual operation was performed, but when the patient was again seen by Dr. Sands, about a month ago, very much the same condition existed as before, and there was little, if any motion, the arm being almost useless. He repeated the operation of resection of the joint nineteen days ago, removing about two inches of bone, and placing the forearm at a right angle. Primary union of the divided soft parts occurred in thirteen days; the

dressings were then removed, exposing the line of the incision. Only one incision was made, situated in the median line. The adhesions were then forcibly broken up, the ends of the bones turned out of the wound, and two inches sawn off. The Esmarch bandage was used. The periosteum was not saved. He thought the subperiosteal flap in these cases is often prejudicial instead of being of advantage, as you can never tell how much bone is going to be reproduced, and the reproduction often causes stiffness of the joint, or limitation of motion, the very thing to be avoided. There were no signs of previous tubercular disease; there were no evidences of disease of the bone. The wound was absolutely dry in thirteen days. During the operation the one to a thousand bichloride solution was used, and the dressings saturated in the same and partially dried.

DR. GERSTER stated that he would venture to say the explanation of the prompt healing of the wound was because there was no tuberculosis present. He had noticed these secondary operations, as a rule, did well, and he thought the explanation might be that in the primary operation muscular planes of connective tissue were opened into almost as a rule; that the wounds therefore were of a rather irregular, sinuous character, drainage difficult, and spreading of inflammation easy, and that these circumstances formed favorable opportunities for suppuration; whereas, when you perform a secondary operation, you work in uniformly dense firm tissues, which give a comparatively smooth capsular wound, the drainage of which is perfect and tends to rapid recovery.

DR. SANDS remarked that he had no doubt that might be one explanation of the successful result. But he had noticed that where operations had been performed upon cicatrized parts, union by first intention was less apt to occur than when the parts were in a normal condition.

DR. STIMSON asked whether he had given up iodoform.

DR. SANDS replied that he had almost done so.

DR. LANGE then read a paper entitled

CLINICAL OBSERVATIONS ON FIBROMYOMATOUS TUMORS OF THE UTERUS.

(See page 651.)

DR. MARKOE asked whether the ovaries and Fallopian tubes were perfectly healthy in the first specimen.

DR. LANGE replied that they were. On presentation of the second specimen he drew attention to the fact that the tumors had grown between the broad ligaments and overlapped the uterus, as it were, so that only a small portion of the cervix was visible.

DR. MARKOE asked whether he had ever tried Schröder's method of closing the stump and removing the elastic ligature.

DR. LANGE replied he had not done so yet.

DR. MARKOE then asked what kind of suture he used.

DR. LANGE replied that it was a coarse silk suture, but that he had not seen any danger arise from the elastic ligature.

DR. MARKOE thought very favorably of this new method, and that to leave the elastic ligature in the abdominal cavity was a serious matter.

DR. GERSTER stated that, from the published reports of Dr. Hofmeier, an assistant of Schröder's in these operations for fibromyomata, a serious objection was the not rare oozing caused by the stitches used to close the stump. Although the gentleman who published the report seemed to belittle this occurrence, he, himself, thought it quite a serious objection, and he did not think the elastic ligature any objection when passed under the peritoneum, any more than the silk ligature; and besides it gave a safer closure of the vessels. It should be remembered that when the elastic ligature is applied it becomes very thin by tension, and so far as the mass itself is concerned does not cause more disturbance than silk.

DR. STIMSON asked how the ligature was passed under the peritoneum.

DR. LANGE replied that he imagined that both broad ligaments being tied, there exists on either side of the uterus, close to its edge, an opening through which the most median ligature of the broad ligament has been passed. Between these two openings the elastic ligature is passed, underneath the peritoneal covering, first on the anterior, then on the posterior side of the uterus, and tied at the point of introduction. He had formerly used a thin leaden ring for securing the elastic ligature, which was tightly squeezed together. Lately he had used a coarse silk ligature for the same purpose. In one of his cases the elastic ligature with lead ring had passed through the cervix as mentioned before.

DR. GERSTER remarked that he would like to bring up a practical question which the general surgeon has frequently to meet, and that is in reference to the length of the incision into the abdominal cavity. He does not consider that the length of the incision into the abdominal cavity materially increases the danger of the operation, but did not desire to enter into a discussion upon the subject, as his experience is comparatively small; but still he could speak from experience of the bad consequences of too small an incision in these cases of laparotomy. He had witnessed a number of ovariotomies in which enormous veins had been torn across during the efforts of extracting a large tumor through a small incision, the resulting hemorrhage being very severe; and the difficulty of finding the bleeding vessel being much increased owing to the smallness of the primary incision, which ultimately had to be enlarged after all. He also quoted one instance in which he witnessed an extensive and fatal laceration of the intestines, owing to forcible attempts at extraction of a large tumor through a small orifice, in what was at first an uncomplicated case. Had a larger incision been made in the first place, he thought the whole of the tumor, together with the adherent gut, might have been taken out of the abdomen without use of force, and the adhesions removed with ease; but owing to the accidental rupture of the intestines, and consequent shock, the patient died as the result of the operation. Did time permit, he could recite several more similar cases.

DR. BRIDDON thought that the statistics of the English ovariotomist, Sir Spencer Wells, relating to the mortality following the long and short abdominal incisions, had exerted an unfavorable influence on the practice of American surgeons; the long incision had really nothing to do with an increase in the mortality, except that it was required in a more formidable class of operations either as regards the size of the tumor or the presence of adhesions; there are many and serious disadvantages in using the short incision.

DR. GERSTER thought it was only a general surgical principle that he adhered to in this respect.

REMOVAL OF PORTION OF TONGUE FOR CANCER.

DR. SANDS presented a specimen of a portion of the tongue removed from a man forty-seven years of age, on account of cancerous disease which began about a year ago. It extended from the tip of the tongue backward, about two inches, involving mainly the right side of the tongue, but on removal was found to have extended deeper into the left side than was at first imagined. The specimen was removed to day by Whitehead's method, the arteries being tied instead of twisted. He thought that the idea of extensive hemorrhage in removal of the tongue is only a bugbear. During the last three operations of this kind which he has performed, the loss of blood did not exceed two or three ounces.

DR. GERSTER remarked that he wished to put on record a case in which he had serious difficulty on account of blood getting into the larynx. It was an operation involving the tongue and floor of the right side of the mouth. The lingual arteries were tied and Whitehead's speculum introduced into the mouth, and he then proceeded to operate in the usual manner. Hemorrhage was slight, but a serious difficulty in respiration was caused by a clot of blood being drawn into the glottis. Alarming asphyxia occurred, and tracheotomy had to be performed rapidly; this at once relieved the patient and respiration was again established; the patient, however, died of exhaustion due to acute mania at a time when the wound was nearly healed. In another case, occurring shortly after the first one mentioned, he performed preliminary tracheotomy, using the tampon canula; and the facility with which the operation was carried on without danger of asphyxia was marked.

DR. SANDS thought that in these operations about the mouth partial anæsthesia is better, as the patient then has the power and will to eject anything from the larynx should there be danger of asphyxia.

DR. STIMSON asked whether cocaine was used, and how.

DR. SANDS replied that he injected it along the gustatory nerve, the anæsthesia produced being very great, fifty minims of a four per cent. solution being used. Six injections were made altogether. He thought the use of cocaine in these cases is of signal benefit.

DR. BRIDDON stated that he nearly lost one case of this class during the operation, and thought that Nussbaum's method of anæsthetizing the patient is very good in these cases.

DR. MARKOE presented a patient in whom he had used

WIRE SUTURES FOR UNUNITED FRACTURE OF THE CLAVICLE.

The fracture had been of some months' standing, and in March last when the young man came under Dr. Markoe's observation, it was found that he was suffering from an ununited fracture of the clavicle, being unable to raise the arm up to more than in line with the shoulder. The power of lifting with the injured arm was very greatly impaired.

On March 17th he was operated on, the fractured ends of the bone being sawed off so as to present a fresh surface and clean section; these were then wired together with silver wire and dressed antiseptically; the result being an excellent union of the bones and perfect restoration of the functions of the shoulder-joint, the strength of the arm constantly increasing. He can now raise his hand to the top of his head and use a brush or comb. There was slight suppuration caused by the silver wires and they were removed; after their removal the wound healed. Dr. Markoe stated that the only case of wiring of ununited fracture of the clavicle that he could find recorded was reported by Mr. Barker to the Clinical Society of London, in December, 1885.

PERMANENT CURE OF STRICTURE OF THE ŒSOPHAGUS.

DR. SANDS called attention to his paper read before the Society in October of 1884, upon "Stricture of the Œsophagus," quoting as an instance the case of a young girl. He thought that it had been the general impression of late years that stricture of the œsophagus could not be permanently cured, our efforts being confined to affording temporary relief. But in this instance he would like to have it put on record, that the relief then afforded has remained permanent up to the present time, although the stricture was excessively tight. A letter just received from the father of the child referred to stated that: "Addie is quite well. It is a little more than a year since the bougies were inserted. She eats without the slightest difficulty any kind of food as well as I can, and has not had within that time the slightest perceptible trouble."

NEW INVENTIONS.

A FORM OF PLANE MIRROR, FOR THE SHADOW-TEST.

BY EDWARD JACKSON, M.D.,
OF PHILADELPHIA.

THIS method of measuring refraction may be practised with almost any piece of mirror-glass, with a sight-hole scraped in the silvering. But for convenience and accuracy the mirror represented, of full size, in the accompanying cut, is much to be preferred.

The sight-hole being cut through the glass and the edges blackened, reflections are avoided, which when present are very apt to mask the light-area in the pupil, making the movements difficult to see, as the point of reversal is approached.

The mirror is one and a half inches in diameter. When a narrower mirror is used, the illuminated area on the face, under certain conditions, passes entirely off the eye before the light-space in the pupil has moved far enough to indicate distinctly whether that motion is direct or reversed. This is, of course, very confusing to the observer. It is quite apt to occur when the segmental tilting mirror, usually furnished with the best American refraction ophthalmoscopes, is employed for this test. The width of one and a half inches is shown, both by calculation and trial, to be sufficient under all usual conditions.

The straight cylindrical handle is adopted because it enables the surgeon to rotate the mirror exactly about a definite axis; and in measuring astigmatism it is extremely important to secure this accurate rotation about

axes perpendicular to the planes of the principal meridians of refraction.

The necessary movements of rotation being but slight, the handle is made comparatively thick, so as to give

better control of these slight movements. In using the mirror, the axis of rotation is always the axis of the cylindrical handle.

The mirror is made by James W. Queen & Co., of Philadelphia.

NEWS ITEMS.

THE PENNSYLVANIA STATE MEDICAL SOCIETY held its annual session at Williamsport, on June 2d, 3d, and 4th, Dr. E. A. Wood, of Pittsburg, presiding. The special interest of the meeting centred around the delegation of the Philadelphia County Medical Society, against whose admission a protest had been filed, similar to that which had been presented at the meeting of the American Medical Association at St. Louis.

Dr. Helsby, of Williamsport, Chairman of the Committee of Arrangements, announced that a protest had been received, and the President stated that, in accordance with the precedent established by his predecessors, the delegates would not be allowed to take their seats, and the protest would be referred to the Judicial Council.

Dr. Dulles, of Philadelphia, then offered the following resolution:

Resolved, That it is the sense of the Society that when a protest is presented against the admission of delegates from county medical societies, this shall not act as a bar to their registration, but that they shall be registered upon presentation of such certificates as are provided for by Article III. of the Constitution, and not be deprived of the rights of delegates until the charges of the protest are established.

The adoption of the resolution was strenuously opposed by Dr. Pancoast, and advocated by Drs. Dulles, Keen, and H. C. Wood. Finally, when a vote was taken, it was adopted almost unanimously.

On the second day the Judicial Council reported that it had decided, after careful investigation, that the grounds of the protest were not sustained, and that the delegates of the Philadelphia County Medical Society were legally elected, and were entitled to seats in the Convention. This announcement was greeted with prolonged applause.

The following officers were elected for the ensuing year:

President.—R. Davis, M.D., of Wilkes-Barre.

Vice-Presidents.—Hobart Allport, M.D., of Phillipsburg; Isaac N. Kerlin, M.D., of Elwyn, Delaware Co.; D. J. McKibben, M.D., of Ashland; and W. B. Lowman, M.D., of Johnstown.

Judicial Council.—John H. Packard, M.D., of Philadelphia; John Curwen, M.D., of Warren; and A. H. Schaffer, M.D., of Mifflin.

The Society elected as its delegates to the American Medical Association all the delegates of the Philadelphia County Medical Society who were refused admission at St. Louis, and who, by virtue of their presence at Williamsport, were eligible for election.

It was decided to hold the next meeting at Bedford, and Dr. A. Enfield was made Chairman of the Committee of Arrangements.

INOCULATION EXPERIMENTS WITH RABIES VIRUS AT BALTIMORE.—DR. GEORGE M. STERNBERG informs us of the following interesting facts and experiments:

Mr. H., aged forty-seven years, residing in Brooklyn, N. Y., was bitten by a dog May 6th, was taken sick May 12th, and died May 14th, with pronounced symptoms of hydrophobia. The dog which inflicted the bite was shot, but not examined, and there is no evidence that he was rabid other than the death of Mr. H., which, in my opinion, furnishes proof positive that such was the case.

The fact that Mr. H. died of hydrophobia is established by experimental evidence. A post-mortem examination was made by Dr. Spitzka, and through his courtesy, and that of Dr. Van Cott, of Brooklyn, I received portions of the brain and spinal cord. On Monday, May 17, I inoculated three rabbits by trephining, according to Pasteur's method. One died the following day from the operation. The others remained in good health until Wednesday, June 2, when symptoms of paralysis of the posterior extremities were observed. The following morning the posterior extremities were completely paralyzed. This paralysis continued to progress until the animal was completely helpless. The symptoms were precisely those described by Pasteur, and exactly such as I had observed in his laboratory last spring. The animal was on the point of death on Saturday evening, and I killed it and inoculated two more rabbits with material from the medulla oblongata. The companion rabbit first showed signs of paralysis in

the posterior extremities on Saturday evening. This morning (Monday) it is completely paralyzed posteriorly, and undoubtedly has hydrophobia as a result of the inoculation made. One rabbit was inoculated with cord, and one with material from brain of Mr. H. The prolonged period of incubation—sixteen days in one case and nineteen days in the other—may be due to the fact that the material used was already two days old when the inoculation was made.

THE NEW YORK CHAMBER OF COMMERCE ON THE DISINFECTION OF RAGS.—The New York Chamber of Commerce adopted on June 3d the following opinion:

1. That invoices of rags accompanied by a certificate of the United States Consul that they were all gathered in a country which at that time was, and for at least six months immediately preceding had been, free from cholera, or other contagious or infectious epidemic diseases, should not be required to be disinfected either before shipment or on arrival. In the absence of such certificate, rags coming from a port or country claimed to be free from such diseases should be disinfected on arrival.

2. That rags gathered in any port or country where such diseases then were, or had been prevalent at any time within six months immediately preceding the shipment thereof, and whether such rags are exported direct, or via any other port or country, and whether landed or stored therein, or transhipped overside to another vessel for reëxport, should be prohibited from being landed in the United States so long as any prohibitory order, published by the Government or health officers, remains in force; nor when the order is rescinded should such rags be landed until disinfected on arrival.

3. That quarantine regulations in relation to rags or other merchandise, or the disinfection thereof, should be uniform at all ports of entry in the United States, and should be under the sole control and management of the United States Government.

CHOLERA IN VENICE AND FLORENCE.—The Marine Hospital Bureau has received information from Venice, under date of June 6th, to the effect that during the last fourteen days there has been a daily average of 41 cases and 22 deaths from cholera; the maximum number of cases being 61 with 36 deaths. The same bureau has also received information from Florence, Italy, dated June 3d, that two deaths have occurred there from cholera and more expected.

GERMAN GYNECOLOGICAL CONGRESS.—The first meeting of the German Gynecological Society will be held in Munich on the 17th, 18th, and 19th of June, 1886.

MEDICAL SCHOOLS IN JAPAN.—Japan has been mapped out into six divisions, and a medical college is to be established in each division.

VENTILATION OF SEWERS BY CONNECTION WITH TALL CHIMNEYS.—R. M. Bancroft writes, in *The Sanitary Engineer*, that in England there are about twenty towns where some of the sewers are ventilated by means of tall chimney shafts. The owners of the chimneys give permission to the local boards or town authorities for connecting the sewers with the chimneys, and in many instances the draught of the latter has been improved. It is queried whether this method has been tried in the United States, and with what results.

PROCREATION AT A PREMIUM.—A decree was recently enacted in France, reaffirming the law of the 29th Nivôse, year XIII., according to which every father of a family having seven living children may have one of his sons educated at the expense of the State. The object, of course, is to increase the population of the country, and the immediate occasion of the degree was the publication last year of statistics showing a large falling off in the average size of families in France.

NOTES AND QUERIES.

CORRIGENDUM.

Page 624, twelfth line from top of first column, *for* "a level teaspoonful," *read* "a level teaspoonful and a half."

Page 624, first line from top of second column, *for* "two ounces," *read* "one ounce."

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT OF THE U. S. ARMY, FROM JUNE 1 TO JUNE 7, 1886.

BAXTER, J. H., *Colonel and Chief Medical Purveyor.*—Ordered to proceed to New York City on public business, and on the completion thereof to return to his station.—*S. O. 128, A. G. O.*, June 3, 1886.

PAGE, CHARLES, *Lieutenant-Colonel and Surgeon.*—Granted leave of absence for one month, with permission to apply for ten days extension.—*S. O. 55, Department of Missouri*, June 1, 1886.

ADAIR, G. W., *Captain and Assistant Surgeon.*—Granted leave of absence for two months, to take effect when his services can be spared.—*S. O. 128, A. G. O.*, June 3, 1886.

GARDNER, E. F., *Captain and Assistant Surgeon.*—Ordered for duty at Madison Barracks, New York.—*S. O. 49, Division of the Atlantic*, June 1, 1886.

CARTER, W. F., *Captain and Assistant Surgeon.*—Ordered for duty (temporary) at Fort Concho, Texas.

Par. 2, *S. O. 55*, Department of Texas, *c. s.*, granting Assistant Surgeon Carter leave of absence for one month, is revoked.—*S. O. 46, Department of Texas*, June 1, 1886.

ROBERTSON, R. L., *First Lieutenant and Assistant Surgeon.*—Relieved from temporary duty at Fort Snelling Minn., and ordered to Fort Keogh, M. T.

PHILLIPS, JOHN L., *First Lieutenant and Assistant Surgeon.*—Relieved from duty at Fort Keogh, M. T., and ordered to Fort Lisston, D. T.—*S. O. 45, Department of Dakota*, May 24, 1886.

BLACK, C. S., *First Lieutenant and Assistant Surgeon.*—Ordered for duty at Fort Stockton, Texas.—*S. O. 64, Department of Texas*, June 1, 1886.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY FOR THE WEEK ENDING JUNE 5, 1886.

SMITH, HOWARD, *Surgeon.*—Detached from the "Nipsic," and placed on waiting orders.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course, not necessarily for publication.

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